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Psychological Bulletin

THE EFFECTS OF PSYCHOLOGICAL STRESS UPON PERFORMANCE¹

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An understanding of the effect of psychological stress upon skilled performance is of great theoretical and practical importance. People are often faced with the necessity of performing skilled work under conditions which are highly stressful. Such is obviously the case in military combat. The effectiveness of a pilot, gunner, or radar observer must be maintained even when he is threatened by physical injury or harassed by the need to hurry the performance of a complicated task. The obvious fact that human beings are often required to work under stress does not call for further elaboration.

The problems of stress involve questions of emotions, motivation, and learning. Many theoretical issues in these fields are of basic importance in an analysis of the effects of stress upon performance. This fact has been recognized; most of the experimental work upon stress has been undertaken for theoretical rather than practical reasons. The problem of the effects of stress cuts across many fields. We are able to draw many hypotheses concerning stress from the theoretical constructs of motivation, emotion, and learning.

We shall begin the discussion of the work in this field with an analysis of the concept of stress. Following this we shall describe the various experimental techniques used to induce stress. This aspect of the problem is important because there are many methodological difficulties inherent in the production of a genuinely stressful situation. Then we shall review, briefly, the kinds of performance which have been studied under stress. Finally, we shall discuss some of the theoretical implications of the work on stress and skilled performance.

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THE CONCEPT OF STRESS

It is not possible to discuss intelligently the work on psychological stress without dealing with the problem of the concept of stress. The definitions of stress that have been given from time to time are inadequate for several reasons. It is possible to think of stress in terms of situations. For example, we say that a crucial examination is stressful to the participants, or that combat is stressful to soldiers. One difficulty with this approach is that these situations are not reacted to uniformly by all people. We cannot predict the behavior of individuals by simply describing the situation. One person may tremble, sweat, experience discomfort, and show signs of behavioral disorganization. Another may show an impairment in performance with no other subjective concomitants. Still others may show no measurable effects from the situation.

In most of the research on stress, the experimenter selects a situation which, from past experience, seems to be threatening to most people. Implicit in this selection is the necessity of identifying stress with the motivations of the people who are being tested. However, because people differ in motivations and in the ways they deal with them, it is never really possible to define a general stress situation. The situation will be more or less stressful for the individual members of the group, and it is likely that these differences in the meaning of the situation will appear in terms of performance.

It is also possible to define stress by emphasizing the reactions or responses of an individual rather than the situation. The trouble with this approach is similar to that encountered when we emphasize the situation. What kinds of reactions should we measure? Are we to consider changes in skilled performance as the "measure" of stress, or are we to consider changes in subjective report? It is apparent that these things may change independently of one another. Moreover, these changes are a function of many unrelated variables. For example, skilled performance may be affected by a change in motivation or a change in approach to the task. It would be meaningless to identify these changes as the effects of stress.

Since stress cannot be defined in terms of stimulus or response operations alone, it is necessary to think of it in terms of an intervening variable.² The additional concept that is necessary is that of motiva-

² Since the submission of this manuscript an interesting article by Brown and Farber, entitled "Emotions conceptualized as intervening variables with suggestions toward a theory of frustration," has appeared in this JOURNAL (November 1951, 48, 465-495). In it Brown and Farber offer some opinions which appear to be parallel with, and certainly related to, our theoretical discussion of psychological stress.

tion. Stress, therefore, is really a secondary concept, built upon the relationship between a primary concept, motivation, and the situation in which motivated behavior appears. We would then think that stress occurs when a particular situation threatens the attainment of some goal. The actual responses that the individual may show will depend partly upon the kinds of mechanisms that have been previously established.

This viewpoint demands that the concept of motivation itself be explored. The psychologist who is interested in problems of human behavior finds it very difficult to estimate from measures of behavior the kind and degree of motivation involved in a particular situation. There is general agreement among psychologists that it is ultimately essential to do this in order to account for the enormous individual differences that are found in behavior. In studies of psychological stress individual differences tend to be one of the main findings.

Pointing out the parallel between the problems of psychological stress and those of physiological stress will illustrate the difficulty in dealing with motivation. Physiological stress does not seem to involve the same definitional problems that psychological stress does, because the "motivational component" in physiological stress is stated in terms of the well-worked-out mechanisms of homeostasis. Selye (34) has defined physiological stress as any condition that produces the "adaptation-syndrome," which is the reaction of the organism in returning to the homeostatic state.

The psychologist has no adequate way of defining the psychological condition that corresponds to the homeostatic steady state. Consequently, the use of the term *stress* must necessarily be a little looser than we would like it to be. When we speak of tension-systems, what we are really doing is postulating a psychological steady-state as a lack of tension. What needs to be investigated are the properties of such a state and deviations from it.

The solution of most experimenters who have studied the responses of groups under stress has been to produce situations which are thought to thwart the motives of most people. This is an adequate solution as long as one is not attempting to account for the reactions of any individual. If the experimenter tries to account for a particular individual's response, this assumption is not satisfactory.

EXPERIMENTAL PROCEDURES FOR PRODUCING STRESS

The principal problem in the study of behavior under stress has been the production of realistic stress situations. A variety of techniques have been tried. Indeed, it might be said that no two experimental

studies in the literature exactly duplicate the same technique. This variety of method has led to considerable confusion, since it is likely that each of these techniques has a somewhat different effect upon performance. It is important to review the main techniques that have been used. These techniques fall into two main classes: (a) stress induced through failure, and (b) stress induced by the task itself.

Stress induced by failure. Failure or threat of failure at a task has been the method most frequently used in experiments on stress. This has been specifically done in the following ways:

1. By presenting the subject with an unsolvable task. In this procedure the subject must work at a task that, unknown to him, cannot be solved. There are a number of ways in which this condition may be established. For example, anagrams for which there are no solutions may be mixed with a group of solvable ones. Or, in another case, a subject may be required to retain a list of digits which is beyond any individual's memory span.

2. In another type of failure-stress, the subject may be interrupted at the task before he could possibly have finished. The task may consist of a group of arithmetic problems. Before he has finished all of the problems, he is interrupted, and told that his time is up.

3. A common technique for the production of failure-stress is the introduction of false norms which indicate failure even if performance has been adequate. The use of this technique may incorporate aspects of the first two procedures. For example, an individual may be interrupted before he has completed all of the items on an arithmetic test, and then told by the experimenter that anyone who has not finished has done poorly. The subject may be told at the outset of the experiment that anyone with normal intelligence (or anyone who expects to be successful in college, or have a decent career in the Air Force, etc.) ought to be able to complete this task within the time limit. In the latter case, failure to complete the problems automatically tells the subject that he is doing poorly.

These techniques of failure-stress present some difficulties. One of the most important experimental limitations is the problem of the control of the subject's motivation. In order for a failure situation to be stressful it is necessary for the individual to be motivated to succeed, or at least to avoid failure. While most subjects are anxious to succeed, the real problem is that all subjects are certainly not equally anxious. Some may not be motivated at all. The usual assumption is that enough of the subjects will be sufficiently involved to become stressed. Some subjects will be seriously disturbed by the threat of failure, while others may be scarcely threatened at all. Of course, the effect of stress will depend upon what the individual expects or demands of himself.

Unfortunately, there is no way of assessing with any confidence the degree of motivation of subjects. We cannot take the effect of stress on performance itself as an indicator of the intensity of motivation because the amount of motivation is not the only determiner of the extent and direction of the reaction to stress. It is certainly to be expected that in some instances strong motivation may produce better performance. On the other hand, it is also capable of producing impaired performance. For the moment, the question of motivation in failure-stress experiments is an important, unsolved problem.

A second difficulty in the failure-stress procedures is presented by the degree of realism that can be produced. In order to be genuinely stressed, subjects must be convinced that the instructions and information given by the experimenter are *bona fide*. Usually the experimenter trusts to his ingenuity and assumes that his instructions will be accepted at face value. There are very few experiments which attempt to assess, directly or indirectly, the degree to which the subjects accept the situation as genuine. There can be no doubt that in most of the experimental situations, the subjects' reactions to the experiment vary from skepticism to severe ego-involvement.

Finally, a seldom considered difficulty in the use of failure-scores to produce the stress is the extent to which a subject will use this false information to alter his mode of attack on the task at hand. Changes in behavior toward the task as a result of failure-information may sometimes have little or nothing to do with stress itself. A subject who has been doing an excellent job may be encouraged (particularly if he is adaptable) to give up a fruitful mode of attack on the grounds that it has been unsuccessful. Thorndike pointed out long ago that an experience of failure may make a subject more variable in performance on the basis of the information it gives him.

Stress induced by working conditions and the task itself. In addition to the experiments in which stress is produced through failure, pressure on the subject may be induced by manipulating the situation in various ways so as to produce excessive demands upon him. Various forms of distractions may be included in this category. Almost any strong sensory input which is extraneous to the task at hand may serve as a source of distraction. In an experimental situation it may be an electric shock, noises, or flashing lights. Distraction can also be produced by verbal disparagement of the subject's performance by the experimenter. Such disparagement may serve as a failure-stress as well as a distraction. In some experiments it is impossible to assess the role of verbal disparagement because it is introduced while the subject is performing, and is therefore both a threat of failure and a distraction.

We have suggested that the manipulation of the experimental situation along the lines of distractions may make difficult demands on the subject, and that these demands then serve as sources of stress. Moreover, some tasks themselves appear to be inherently stressful for much the same reasons. The task may require the subject to attend to too many things at a time, or to perform too many operations at once. These demands may be increased by the experimenter as in the case of rapidly pacing the subject. In some ways this type of stress is like the situation a person faces when he is learning to drive a car. The stress here is probably less a matter of failure—although in some instances it could be—and more of making too many demands at once upon the learner. In other cases there is the added possibility of the threat of personal injury or damage to the car. We would class this stress situation as one induced by the working conditions and the task itself.

We have contrasted these two basic types of stress situations, failure-stress and task-induced stress, because the problem of motivation appears to be a little different for each of them. In the task-induced stress, the motivation depends primarily upon how the subject interprets the experiment. If he sees the distraction or pacing as "something to beat," his effort level is apt to be raised. Motivation can be roughly equated for all subjects by carefully wording the initial instructions, or can be left to vary from subject to subject by ambiguous information. Consider, for example, the effect of instructions which indicate that the purpose of the experiment is to see how well the subject can hold up under distraction as compared with the introduction of distraction as a natural part of the experiment or the task so that the subjects will not react to it as something to be overcome. Despite the methodological problems raised here, the problem for the experimenter is simpler in the task-induced stress than it is in the failure-stress experiment. The failure-stress situations depend primarily on the production of a realistic threat to the subjects' self esteem or to some goal-oriented behavior. The variability of reactions to this kind of condition must be considerably greater than to the situation where stress is induced by manipulation of the working conditions or by the excessive demands of the task.

Evaluating the differences in technique. Unfortunately, we can only guess, at present, what the effects of the differences in the techniques of production of stress might be. Most of the experimental situations which have been used in the study of stress are not directly comparable with one another. It is almost impossible to generalize with confidence from the study of the effects of any one experimental stress-producing technique. One of the important problems for future research is the

study of the differences in the effects of the various procedures for inducing stress.

THE MEASUREMENT OF THE EFFECTS OF STRESS

One of the chief difficulties in the design of stress experiments is concerned with the measurement of the effects of stress upon performance. It is important to measure the effects of stress unconfounded with any other variable. The problem arises because it is often very difficult to get a measure of reaction to stress which is independent of the subject's ability to learn or perform certain kinds of tasks.

For example, if the experimental problem is to study the effects of some kind of stress upon the learning of a psychomotor skill, the question arises, "How can we differentiate the subject's 'normal' rate of learning from his rate of learning as it is affected by the stress condition?" For data which deal with groups of subjects there is a simple solution to the difficulty. It is possible, through control subjects, to estimate the typical rate of learning and compare it with the rate of learning of a comparable group of subjects under some stress condition. If the experiment is properly designed, and the control group appropriately selected so that it is adequately matched with the experimental group, then we can observe the general effects of stress as a difference in the mean performance or a difference in the variability of performance between the two groups.

The tricky problem actually arises when we are concerned with the performance of a particular individual under stress. Whenever we are engaged in predicting performance under stress from personality measures, it is necessary to obtain a measure for that particular subject. This "stress score" must be measured independently of the subject's initial ability and his change in performance due to fatigue or learning. Because individuals differ so much with respect to their basic abilities and rates of change, some estimate of each subject's performance without stress is necessary before correlational techniques may be used.

It has been possible to solve this dilemma in three ways. The most obvious technique has been to test each subject twice—a pretest under standard conditions followed by a stress test. In that way, a difference score may be obtained between performance under stress and performance without stress. If this difference score correlates with the score on the first test (as it usually does) then it is necessary to correct this difference measure so that it is free from the subject's level of performance on the first test. It is a statistical fact that subjects who do well on the first test tend to go down on retest, while subjects who do badly

on the first test improve on the second test. This means that difference scores cannot be used to represent stress scores without a correction for regression on the first test. When this correction has been made, the resulting score represents the effect of stress upon performance for any one subject.

Another technique for obtaining individual stress scores is to make use of plateaus in the learning curves, or to select tasks for study that show little or no change in performance with training. In the case of plateaus, if a subject has attained a point in the learning of a skill where little or no further change in performance is expected for some time, this level might be used as a reference point against which to compare his performance under stressful conditions. Or the plateau may be induced by giving a subject so much practice that he attains his maximum achievement on the task, and no further changes are expected.

The trouble with plateaus is that they are seldom very dependable. Most plateaus are illusory, or cannot be depended upon to remain steady long enough to introduce the stress condition satisfactorily. In the case of the final plateau, where subjects have been practiced to a maximum of performance, this level is often really not a true maximum, and further practice produces a spurt in performance. Furthermore, this latter technique, used by Williams (43), has the disadvantage of producing a stress situation which can only result in a decrement in performance if the subjects have reached their physiological limit.

In summary, it may be said that it is necessary to provide some kind of base line or standard of comparison against which to evaluate the uncontaminated effects of stress upon performance. This reference point must be made free of the influence of the subjects' abilities in the skill in question. To do this, the technique which appears to present least difficulty makes use of two test periods with each subject, one under stress and the other under control or standard conditions. We should point out that even this technique is by no means ideal. Difference scores suffer from the difficulty of requiring two equivalent tests for the same subject. Moreover, it may be important which is given first, the control or the stress condition.

A third approach is possible, but so impractical that we shall dismiss it quickly. If it were possible to match individuals beforehand on ability to learn or perform the skill, then one of the pair could be given to the control condition and the other to the experimental condition. Pretests actually could be used to match subjects. However, this technique has not been used because it would be necessary to match not only initial score, but also rate of learning, approach to the task, motiva-

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EXPERIMENTAL STUDIES OF PERFORMANCE UNDER STRESS

For convenience, the experimental studies have been classified into those which deal with verbal tasks and those which deal with perceptual-motor tasks. This division is somewhat arbitrary, but follows along more or less traditional lines. It is possible that a more rational classification may arise out of further experimental work.

The effects of stress upon verbal performance. Several experimenters have studied the effects of stress upon intelligence, primarily verbal, in children. Using nine-year-old boys, Lantz (15) obtained a statistically significant impairment of Stanford-Binet scores following a failure experience, but no such effect after a successful experience. An examination by Lantz of the differential effects of this failure experience upon the various subtests indicated that tasks requiring visual or rote memory were not affected, while those involving reasoning or thinking suffered a decrement.

Along similar lines, an interesting experiment by Hutt (13) differentiated between the effects of failure-stress upon a group of maladjusted children and a group of well-adjusted children. Two methods of administration of the Stanford-Binet were used: (a) an adaptive procedure, in which a failed item was followed by one upon which success was likely, and (b) the standard method, which begins with easy items and ends with items which will be failed. The adaptive method, which produced greater psychological support, resulted in higher average IQ's for the maladjusted group. No difference between the two testing methods was found for the well-adjusted group. This study has considerable implication for the problem of stress in ordinary psychometric procedures.

The great majority of the studies of stress and verbal performance show deterioration or impairment as the result of the experimental conditions. Alper (2) finds a decrement in production in a sentence-formation task as the result of failure-stress. In another experiment, stress induced by pacing and the inherent difficulty of the arithmetic problems produced a reduced rate of learning (40); pacing, in this case, produced failure in 19 out of every 20 trials. Impairment of digit span was obtained with fifth- and sixth-grade children when the number of digits presented was beyond the normal span for these children (44). Zander Zeller (45), working with nonsense syllables, found a decrement in recall and relearning following an experience of failure; he accounted for

this decrement on the basis of repression of the items which subjects associated with failure. His evidence for this hypothesis was that re-learning and recall improved when the knowledge of failure was removed. Zeller's study may be interpreted as basically a stress experiment. Zeller comments that the subjects' performance progressively deteriorated during the accumulation of failure.

In another failure-stress experiment in which nonsense syllables were learned and recalled, Sulliyen (37) found that (a) success produced the most rapid learning and the most complete recall, (b) slowest learning and poorest recall followed failure, and (c) failure was more harmful to a superior group in producing impairment than success was facilitating. The reverse was true for an intellectually inferior group.

Impairment of performance on the digit-symbol test was found by Williams (43) with the use of false failure scores. Williams was primarily interested in validating the Rorschach test. He concentrated upon the prediction of performance under stress by selected Rorschach indices.

In another experiment, stress, induced by failure to complete a task within a specified time limit, produced an increase in errors and variability in time scores of multiplication problems and in the learning of nonsense syllables (21).

Postman and Bruner (30) studied recognition thresholds under stress using three-word sentences. Psychological stress produced by failure and ridicule resulted in poorer performance. Since the stress was administered during actual performance, the results may have been due, in large part, to distraction. The authors observed that during failure, the pre-recognition guesses of the subjects became extremely reckless.

Despite this array of studies showing impairment, poorer performance has not always been found to be the principal effect of stress. Initial decrement but later improvement in code-learning was obtained as the result of stress due to pacing.⁸ Hurlock (12), on the other hand, found that failure results in an initial improvement and then a later decrement. Lazarus and Eriksen (16) found that the major effect of failure-stress upon the digit symbol test was to produce greater variability between subjects; some subjects improved and others showed impairment.

Finally we might note that there have been a large number of studies aimed at the analysis of selective recall or repression that have some bearing on the problem of the effects of stress (2, 6, 10, 14, 24, 31, 45).

⁸ DEESE, J., & BOWEN, H. M. The effect of task-induced stress on code learning. Unpublished research, The Johns Hopkins University, 1950.

Decrement
+ the improvement
Greater variance

Eriksen's data (6) suggest that poor or good recall of incompleting tasks may be a function of personality variables related to defense mechanisms.

The effects of stress upon perceptual-motor performance. The general picture of the effects of stress upon perceptual-motor performance is similar to the pattern found with verbal tasks. A large number of studies have shown impairment of perceptual-motor performance under stress. Marquart (23), with the use of visual discrimination, concluded that frustration resulted in slow learning, increased rigidity, and non-adaptive behavior. McClelland and Apicella (20) used card sorting as the experimental task and found that stress induced by false failure-scores resulted in more trials before the criterion was reached. Longer reaction times for completing pictures that were flashed on a screen were obtained by Verville (41) when subjects were given unsolvable tasks prior to the measurement of reaction time. Moreover, slower reaction times were also found by Verville for subjects who were required to solve a number of difficult problems simultaneously before the test of reaction time.

Bayton and Whyte (4), who used a rate of manipulation test, found that a success-failure sequence produced poorer performance than a sequence of failure followed by success. Because they failed to include a control group, it is impossible to decide whether the differences in performance between the conditions are due to success, failure, an interaction between these, or sampling error.

Seashore and Bayelas (33) found a decrement in mental age scores on the Draw-a-Man test under conditions of frustration. The subjects were children of varying ages. In a famous experiment designed to get at the relation between frustration and regression, Barker, Dembo, and Lewin (3) demonstrated a regression in mental age of young children as a result of stress.

One of the most intensive attempts to evaluate the use of stress as a psychometric device for personnel selection was undertaken by the Aviation Psychology Program of the Army Air Forces (27). With the use of various psychomotor tasks such as steadiness, aiming, etc., the effects of verbal threat and distraction upon performance were measured. Since this program was primarily concerned with prediction of success in training schools, very little systematic information concerning the direct effects of stress upon the measured performance is available. Many of the studies were conducted without adequate control groups. However, in our analysis of these data (27), the following facts seem to be revealed: (a) Where it can be established that stress produced an

effect, the effect seemed to be a small decrement in performance. (b) An adaptation-effect to the stress condition is suggested by the fact that the stress tests were the only psychomotor tests which produced different results according to their position within the batteries of tests. The scores for the stress tests were poorest when they occurred first in the battery.

more speed less accuracy
Speed & variability
In psychomotor performance, as well as in verbal performance, impairment of ability has not always been found to be the only result of stress. Lindsley (18) found that stress induced by rapid pacing caused the subjects to attempt more problems, which resulted in a greater number of errors and more variability between subjects. Total scores remained approximately the same because the increased speed was offset by an increase in the number of errors. Further data in the type of experiment described by Lindsley were obtained by McKinney *et al.* (22). Two kinds of reactions to stress were found: most subjects speeded up performance at the expense of accuracy and were more variable, while a few subjects showed a stable performance without any increase of speed or errors. As a whole, the stressed subjects showed a reduction of efficiency in performance in terms of the percentage of correct items to the number attempted. Clark and McClelland⁴ reported that subjects with mild fear of failure performed better, particularly towards the end of a test. Finally, improved performance under failure-stress was found by Gates and Rissland (9) with the use of motor coordination and color naming tasks.

The effects of stress upon the components of performance. Most of the studies of stress report findings concerning the total scores on the performance of some task. There is usually no attempt made to analyze these total scores into their various components and to observe the effects of stress upon them. Two subjects could obtain identical scores on a task as the result of very different patterns of performance. This is especially true where scores may be analyzed in terms of speed and accuracy and where an increase in speed may go along with a decrease in accuracy. A report in terms of total scores may obscure what is really happening to the subject's performance.

Only two studies to date have presented data relevant to this problem. In both of these studies an increase in errors was accompanied by an increase in speed as a result of stress (16, 18). In one of these studies, however, speed accounted for so much of the variance of the total scores

⁴ CLARK, R. A., & MCCLELLAND, D. C. A factor analytic integration of imaginative performance and case study measures of the need for achievement. Unpublished research, cited in McClelland (19).

that any increase in errors was ineffective in changing the total score (16). It is probable that the effects of stress upon such components as speed and accuracy may be very different from task to task.

Adaptability to the experimental conditions and various other less easily measured aspects of performance may also be differentially affected by stress. One problem in this area is the difficulty of determining which measure represents the subject's efficiency in any particular task. In some situations accuracy may be of greater importance than speed, while in others rapidity of performance may be the important feature.

Qualitative observations of performance under stress. In connection with experimental studies of the effects of stress upon performance, many qualitative changes in behavior have been noted. Stereotyped responses (23), inattention (44), disorganized activity (36), and increased overt activity (8) have been some of the aspects of behavior under stress that have been observed. Many experimenters have reported signs of emotional upset such as sweating, tremor, subjective anxiety, pulse changes, etc. A whole new theory of the physiology of the organism (34) has grown up around the organic changes observed as the result of physiological and psychological stress. *Selye*

Investigators have attempted to make field observations on the effects of stress upon performance by means of questionnaires, interviews, and rating scales. These studies have the merit that they attempt to discover the effects of non-laboratory stresses upon behavior. However, they have the great disadvantage of depending entirely upon self-report techniques. The most serious difficulty with these studies is that they do not include actual, objective measurements of performance. The questionnaires are always administered after the stressful situation has passed away, sometimes years later (5).

Dollard (5) administered a series of questionnaires on fear in battle to veterans of the Abraham Lincoln Brigade of the Spanish Civil War. These questionnaires suffered from too many limitations to be of much use. The techniques of sampling, question-wording, the nature of the attitudes measured, etc., make this study difficult to evaluate. The data do tell us something about the conditions which produce stress in actual combat and some of the personal reactions to these stresses. However, there is no way of knowing how the intense fears reported by Dollard's subjects affected their performance.

A somewhat more satisfactory questionnaire approach was attempted by Shaffer (35), with the use of veteran AAF flying personnel. Following an analysis of the results of his study, Shaffer (35) concluded that the adequate stimulus for fear is a highly motivated situation

towards which the individual has no adequate means of adjustment. This definition seems to fit most of the experimental studies of behavior under stress.

Shaffer reported many different signs of emotional upset in combat. Some of his data suggested that the severe stress of battle may, indeed, have a serious detrimental effect upon the various skills required in combat. Subjective feelings of fear increased with the number of missions that were flown, though the pilots questioned believed that this was probably not accompanied by an increase in the deterioration of behavior. Somewhat greater faith can be placed in the information from this report because of the cross-checks, the closer proximity in time of the interviews to the experience in question, and the somewhat more adequate control of sampling. Nevertheless, Shaffer's study still suffers from a complete dependence upon self-report.

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The studies of Shaffer are the only field observations which actually attempted to assess the effects of combat stress on performance through self-report. The statement of a majority of the men questioned was that fear increased their efficiency or at worst made no difference in it. Only from 17 to 31 per cent of the men in the various groups interviewed reported that fear decreased the efficiency of their performance. Those men who reported an increased efficiency as the result of stress said that mild fear increased their efficiency more than intense fear.

The findings of this study are extremely interesting and provocative, but there is a distinct probability that the reports of the men interviewed do not reflect the actual state of their performance under stress. It is likely that the interviewees confused effort with performance. Also, under the circumstances of testing, men might be extremely unwilling to admit behavioral disorganization under combat. The individuals who were interviewed had no good criteria for an evaluation of their own performance under stress. The stressful conditions themselves probably reduced the accuracy of self-estimates of performance. As a result, we suspect that the percentage of men experiencing no detrimental effects of stress was greatly overestimated. In light of the experimental studies on performance under stress, it does seem likely that some individuals will show a facilitation of performance under such conditions, but it also seems probable that these individuals will be in the minority in any randomly chosen sample of people. This interesting problem remains to be studied by more reliable methods.

With the use of narco-analytic techniques, Grinker and Spiegel (11) studied battle-fatigued soldiers of the last war. They suggested that a primary mechanism for psychological breakdown under combat

involves strong guilt feelings and conflict over supposed inadequacy and cowardly behavior during combat. While it is provocative, this study is less relevant here because all of the subjects involved were psychiatric patients.

Personality correlates of behavior under stress. Very little information has been obtained about the relationship between various measures of personality and reaction to stress. The problem has theoretical as well as practical importance. On the one hand, while great individual differences in response to stress have been recognized, few fruitful attempts have been made to discover their nature. On the other hand, it would be most useful to be able to predict which people will be adversely affected by a stressful situation.

Most of the studies which have obtained relationships between performance under stress and measures of various aspects of personality have presented correlations which are statistically significant but are too small to be of practical value. For example, Taylor and Farber (39) found that submissive children showed a decrement in performance and an increase in variability under failure-stress, while ascendant children showed an improvement. In Hutt's (13) study, previously cited, maladjusted children showed impaired mental age scores on a stressful administration of the Stanford-Binet. Lazarus and Eriksen (16) have demonstrated that students with high grade-point averages in college tend to improve under stress while poor students show a decrement and greater variability in performance when ACE scores are held constant. The authors account for this relationship by the suggestion that some poor students may obtain poor grades because of the stressful nature of college examinations.

In a study that is difficult to interpret, Meadow (25) found that women with low dominance feelings were more emotionally disturbed by failure than women with high dominance. In this experiment emotional disturbance was measured by subjective report; the author found that women emotionally disturbed by the failure showed poorer performance in arithmetic and memory tests for emotional words than did those women who reported no emotional disturbance. It is impossible to tell to what extent the subjects' reports of disturbance may have been influenced by their actual level of performance. Individuals who showed poor performance may have attempted to rationalize this by claiming to be emotionally upset during the test. The study, however, is in accord with the results found by Taylor and Farber (39).

The only study that has reported relationships between performance under stress and personality variables of sufficient magnitude to be

of practical use is that of Williams (43). In a validation study of certain variables on the Rorschach test, Williams reported a multiple correlation coefficient of .824 between average decrement under stress and two scoring categories on the Rorschach. There are several features about this study, however, which suggest caution in accepting the results. In the first place, the number of cases upon which the correlations were based was only 25. There are strong possibilities of sampling errors with so few cases. In the second place, the material used was over-learned, so that the scores for the subjects under stress could only decrease. Thus, nothing but "decrement" scores were obtained. In addition, it is to be noted that the stress was produced partly by the distracting influence of the threat of electric shock.

The doubts concerning the generality of the results of this study are supported by negative findings in a study correlating a large series of Rorschach variables with the effects of stress upon performance (7).

Also on the negative side is a study by Adams (1), who found no relationship between neurotic tendency scores on the Bernreuter Personality Inventory and performance under failure-stress. In a similar experiment using the Bernreuter, Marquart (23) also obtained negative results.

McKinney *et al.* (22) found no relationship between attitudes and feelings expressed on a questionnaire and efficiency of performance under stress. Some slight positive relationships are suggested, however, with variability in a few Rorschach responses. However, the measure of variability of these Rorschach responses is not described in the experimental report.

Relevant to the problem of the personality correlates of performance under stress is an experiment by Taylor (38). She found that individuals who show high anxiety as measured by a questionnaire are people who learn a conditioned eyelid response most rapidly. Since the conditioned eyelid response is of the avoidance type, the author concludes that this superiority is the result of a high anxiety drive necessary for rapid avoidance conditioning.

Performance under stress as a predictor. There have been two systematic attempts to use performance under stress as a predictor of success in other dangerous and stressful activity. One was the well-known OSS study, and the other was an attempt by the Aviation Psychology Program to design stress tests.

The more elaborate of these experiments was the program conducted by the Office of Strategic Services to select men who would be most

effective in intelligence operations in enemy territory during World War II (46). The basic procedure was to subject a group of applicants to a series of stress tests designed by the psychological staff of the OSS. The test situations were highly realistic in the sense that they did a good job of simulating stressful situations to which the candidate was liable to be subjected while on duty. The men were aware that they were being evaluated. The stress that was created was basically a failure-stress. All measurements of performance were based upon ratings by continually observing psychologists. While large individual differences were found by the raters, these were not successfully related to later performance in the field. A possible reason for this was that the various subjects in this experimental program served in different theaters of war and performed different functions, so that the criterion of performance was not comparable. The OSS study cannot be considered to have achieved successful prediction of performance under stress, despite its elaborate design.

The Aviation Psychology Program (27) made an attempt to relate performance on a series of psychomotor tests given under stressful conditions to success in pilot training, bombardier training, and navigator training. No significant relationships were found between any one of the five tests used and any available measure of success in training school. Several major criticisms of this study may be given. First, the criteria used were poor ones inasmuch as they were very heavily saturated with semi-academic abilities. Stress may actually lower the validity of a test when the criterion is success in school. Moreover, performance in training school may be poorly related to performance under operational conditions that involve stress. In support of this last statement is a study of the validity of selection and classification tests in the theaters of war. The conclusion was drawn that many of the most important features of combat performance were not measured by the traditional abilities tests (17). Second, the most important factors measured by these AAF stress tests may not be ability to perform well under stress but the specific motor skills called for by the task itself. No attempt was made to measure deterioration under stress, or any other variable that would yield a picture of the effects of stress independent of the individual's ability. This is a problem of experimental design that we have discussed earlier.

It should be mentioned also that attempts to use the selection and classification battery of the AAF to predict the syndrome known as "anxiety-reaction" or "operational fatigue" also resulted in failure (42).

AN EVALUATION OF THE PROBLEM OF THE EFFECTS OF STRESS

The stress experiments mentioned earlier have yielded varying results: some show only a decrement in performance, others show improvement, and still others produce both of these effects for different individuals (an increase in variability). It is important to understand why the results have been seemingly inconsistent.

The distressing fact is that few of the experiments are comparable because they employed different kinds of conditions to produce stress and different kinds of tasks. The question of the dissimilarity of the two large classes of stressful conditions, failure-stress and task-induced stress, has already been raised. No data are available at present that allow us to make any generalization about this question.

Another important problem concerning the comparability of the various experiments cited is that of individual differences and sampling. Various investigators have used children, adults, women, men, college students, and military personnel. Inasmuch as large individual differences have been found in all of the experiments, it is reasonable to suppose that some of the disagreements between these experiments are due to differences in the samples employed. All in all, the body of experimental literature on the topic of the effects of stress on performance is completely unsystematic.

An integrated theoretical picture about the effects of stress upon performance must take account of individual differences, the finding of impairment as well as improvement of performance, the influence of different situations, and the effects of different kinds and amounts of stress. All of the psychological concepts about stress have been motivational in frame of reference. For convenience, we may categorize the explanatory concepts into those which emphasize primarily the energizing and directive aspects of motivation and those which emphasize the emotional aspects of motivation.

The energizing aspect of motivation. Miller (28) and Wickert (42) have emphasized fear as a motivation or drive. Since an increase in motivation is usually accompanied by an increased output in performance, Miller and Wickert both suggest that fear, produced in a stressful situation, may actually be beneficial to performance. They cite as evidence the fact that AAF personnel reported that under the stress of combat they performed more efficiently (42).

This simple notion of fear as a drive may well account for the increased efficiency that one finds occasionally for some individuals under stressful conditions. Few people would argue with the proposition that motivation does increase the adequacy of performance. It is apparent

only up to a
degree

that fear produced by a stressful situation has a considerable motivational component.

The difficulty arises because of the fact that, in some instances, high degrees of motivation or fear seem to produce a decrement or impairment of performance. Neither Miller nor Wickert takes this into account. It is therefore necessary to look for mechanisms that may supplement the notion of the simple, energizing function of motivation. It is possible to think of a critical point in the amount of fear, beyond which disruption occurs.

The directive aspects of motivation. In addition to producing the impetus to action and the persistence of action, motivation has a directive aspect and a terminating effect (26). This means that an individual will direct his efforts towards whatever operations tend to be satisfying. When the motives have been satisfied, the activity in this direction will be terminated. In extremely powerful motivational states, it is very possible that a subject's activity will be unadaptively directed. For example, Lindsley (18) and Lazarus and Eriksen (16) showed that one of the effects of a threat of failure was an increase in the speed of performance as well as an increase in the number of errors. It is quite possible that a considerable portion of the decrements in performance reported in other experiments may be due to an inappropriate increase in speed of performance.

Another illustration of unadaptive behavior as the result of stress comes from an experiment by Patrick (29), who found irrational and repetitive behavior in human subjects who were placed in a problem box and subjected to severe stress.

In many of the experiments in which failure-stress was used, it is possible that telling an intelligent subject that he has done poorly will force him to alter his mode of attack, so that it may be less effective. This kind of an effect could account, at least in part, for impairment found in some of the experimental studies.

An additional way in which the directive aspects of motivation could produce a decrement in performance is the arousal, by threat, of motives that are antithetical to the activities required by the task itself. For example, Rosenzweig (32) suggests that reactions to frustration may be need-persistent or ego-defensive. Need-persistence means that the individual will center his attention and efforts upon the frustrated need (goal-oriented behavior), and ego-defense means that the individual will be primarily concerned with the maintenance of self-esteem.

There are many instances in which activity directed toward the maintenance of self-esteem may make the effective continuance of

performance difficult or impossible. Some writers have referred to this kind of situation as stimulating the individual to leave the field psychologically. The ego-defensive aspects of the situation may become so important to the individual that he ceases or reduces his attack on the task at hand. For example, a subject in an experiment may stop work so that, even if he fails in the eyes of the experimenter, he can justify his failure to himself on the grounds that he really didn't try. The result of this defensive mechanism is most certainly to reduce the person's effort level toward the experimental task.

Thus there are other motives that may interfere with the individual's need to work at and do his best on the task at hand. In situations that threaten physical danger, the motivation to escape may directly interfere with the individual's performance. His continued performance may be, or seem to be, incompatible with self-preservation. A conflict may be produced between the desire for self-preservation and the desire to perform well. This conflict could result in impairment of the individual's level of performance.

The emotional aspects of motivation. Emotional reactions usually accompany very powerful motivation. There is no need to elaborate upon the autonomic components of emotion. The disruptive aspect of emotion, of which the autonomic components are perhaps the most characteristic, may produce mental blocks, tremors, severe anxiety-reactions that make the satisfactory completion of a given task difficult or impossible. Anxiety-reactions may include such effects as nausea, fainting, spasms, weakness, headaches, dissociation, etc. Clinicians have long been aware that the ability of people to retain lists of digits may be severely impaired by the presence of strong anxiety. This recognition finds expression in the diagnostic use of the digit-span test of the Wechsler-Bellevue Intelligence Scale.

One of the effects of anxiety might be to produce a powerful distraction. Threatened subjects frequently report that their productive thinking is disrupted by the compelling preoccupation with the thought of the consequences of failure or danger. We might consider that in some tasks, e.g., those that require fairly automatic responses, this preoccupation would have little effect, whereas in others, e.g., those that require concentration, this preoccupation would be very disrupting.

In the simplest sense, the autonomic overflow in strong emotion would make physical performance difficult or impossible. The individual who collapses before going into combat is protected against the dangers of combat. The person who develops writer's cramp is temporarily removed from the threatening examination. It is difficult to

get adequate subjective reports on the milder effects of autonomic disturbance that may interfere with an individual's performance, because subjects may magnify these effects in order to make use of them as ego-defensive mechanisms. It is possible, for example, that mild autonomic reactions may be accompanied by an improvement in performance, even though the individual may insist that he is too nervous to perform efficiently.

The cause of these disruptive influences probably lies directly with the fear itself, though the impairment may be enhanced by the presence of conflict between the anxiety produced by the threat of danger and the necessity of remaining in the situation and seeing the task through. The disruption may also be increased by the inability of individuals to cope with the fear-producing stimuli. It is possible that situations in which the individual can remove or reduce the stimuli that threaten him may not be so stressful.

Interaction of emotion and motivation. When we try to analyze what is happening in any individual's performance under stress, it is usually almost impossible to separate the effects due to the operation of emotional disruption from those due to the directive characteristics of motivation. Some subjects in an experimental situation may show impairment in performance because of ego-defensive reactions. Some may suffer primarily because of emotional disturbance. Others may be facilitated. Still other cases may involve the operation of both facilitating and disrupting motivational and emotional reactions. In fact these two may balance in any individual to produce no effect upon any measure of performance.

These interactions present a major problem for the prediction of reactions to stress. The same effect upon performance may be achieved via several different routes. Some of the negative findings with respect to the relationships between personality variables and performance under stress may be due to the assumption by many psychologists that impairment is solely related to the lack of emotional control. Because of the multiplicity of causes for any individual's particular reaction to stress, it is not surprising that the scores on inventories of neuroticism and other measures of emotional stability show negligible correlations with performance under stress.

Motivation, emotion, and the kind of stress-situation. We have pointed out that the type of motivation, that is, ego-defensive or need-persistent, the kind of approach a subject makes to the situation, and the effects of strong emotion are all possible bases for an individual's response to the stress situation. It is necessary to point out, however, that the

importance of any of these factors will depend, to a large extent, upon the conditions of the situation and the type of stress involved, as much as upon the personality and past experience of the individual. In other words, ego-defensive reactions are apt to be especially prominent in situations that utilize failure as the source of the stress. On the other hand, distraction situations, which are not accompanied by threats of failure or disparagement, are less apt to make ego-defensive reactions necessary.

Essentially, this boils down to a consideration of interactions between persons and types of stress. It would be interesting to know what kind of individual develops anxiety reactions to task-induced stress. We might guess that such people are highly motivated to perform well. The successful understanding of any individual's performance under stress depends upon some way of measuring the kinds and strength of his motivations and relating them to the characteristics of the situation in which he must perform. The fulfillment of this aim is, indeed, no simple affair.

Task components and the interactions of emotion and motivation. As we have said earlier, relatively little attention has been paid to the study of the components of the total performance of persons in stressful situations. What is going on, in terms of the effects on the subject's performance, may be masked by attention to only the total score. This score is simply the end product of his attack on the problem, his degree and direction of effort, the effects of emotional disturbance, and his abilities.

A consideration of the various components of performance may well yield a clue as to the bases of the observed effects of stress upon performance. For example, periodic blocking in a continuous task could indicate some degree of emotional disturbance in one subject. These blockings might adversely affect speed and regularity and, therefore, result in an impaired performance. On the other hand, in the same situation, another subject might show a greatly increased rate of performance at the expense of accuracy, which would suggest that the energizing and directing aspects of motivation are involved. Both subjects end up with the same total score, for different reasons, and attention to the components of that score would provide some leads about what those reasons might be.

Relating the individual components of performance to possible causes of the stress effects would not be simple. In many instances the relationship may be ambiguous. For example, excess overt activity which has occasionally been reported as an effect of stress would be

difficult to assign to any of these possible causes. Impairment of judgment might reflect emotional blocking or misguided effort as a result of false failure-information. Some help would be obtained, however, by studying the combination of effects, both qualitative and quantitative. Despite the difficulties inherent in the approach we have been suggesting, an attack on the source of the various stress effects and how they operate must be made before we can begin to understand the bases of the effects of stress upon skilled performance. It is our belief that the various individual components of performance can be more successfully related to personality variables than the total score.

Implications for future research. The most obvious questions that require an answer are:

1. How do the effects of psychological stress vary with the nature of the task?
2. What are the differences in effects produced by failure-stress and task-induced stress?
3. How does psychological stress affect the various individual components of total performance?
4. What is the relationship between personality variables, past history, and performance under stress?
5. What is the relation between the capacity of the subject and reaction to stress?
6. What relation do emotion and motivation have to performance under stress?

Research in the area of psychological stress must begin practically afresh. Any systematic program must take into account the difficulty of producing realistic stress situations and making effective measurements of the stress effects which are independent of the skills required by the task itself. It seems certain that a really systematic attack on the problems of psychological stress will produce some valuable answers to both theoretical and practical problems in behavior.

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INFLUENCE OF SOCIAL CLASS ON PERSONALITY TEST RESPONSES

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Do people of different social classes respond differently to psychological tests? Allison Davis and his co-workers have pointedly asked this question and have done extensive research to find the answer (12, 14). They have shown that children of different social classes respond differently to the usual intelligence tests. Middle-class children as a group almost invariably do better on these tests than lower-class children. Not content with knowing that children from different social classes respond differently to these tests, Davis and his colleagues asked, "Are the tests fair to children from all social classes?". Their answer is: the differences between classes are not mainly the result of differences in native intelligence, but reflect a bias of the tests in favor of the middle-class child. Davis and his colleagues have shown that many of the test items deal with matters that the middle-class child has had a chance to learn but the lower-class child has had no experience with or no incentive to learn about.

Now that the work of Davis has opened up the problem of class differences on psychological tests, it is obvious that intelligence tests may not be the only kind of test that would show differences between subjects from different social classes. It is the purpose of the writer in this paper to review the literature on social class differences in responses to personality tests.

If people from different social classes do respond differently to personality tests, it would be necessary to take these differences into account whenever we use personality tests in research or in clinical practice. For example, if we compare Rorschach test responses of children seen at a children's mental hygiene clinic with Rorschach responses of normal school children, the differences between social classes may be confounded with neurotic-normal differences. If the children coming to the clinic are mostly lower-class and the school children are mostly middle-class, the differences between the two classes (if any) are mixed up with the differences between neurotics and normals.

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Differences on personality tests between people of different social classes could be of two kinds: meaningful or trivial. A difference in the way two social-class groups respond to a test may be called "meaningful" when it indicates a true difference on the variable supposedly measured by the test. When two groups respond differently to a test not because they differ on the variable supposedly measured by the test but because they differ on an extraneous variable, the difference may be called "trivial" or "artifactual." Klineberg (23, p. 103) has given an excellent example of trivial test differences:

Some years ago a group of Chinese psychologists applied a number of standard inventories like the Bernreuter, Thurstone, and others to Chinese students and arrived at the sad conclusion that Chinese students were very neurotic. One of the investigators closed with a plea for more mental hygiene in the Chinese universities to overcome this unfortunate situation.

What had happened, of course, was that the specific items in the inventory were interpreted quite differently by the Chinese. One question went like this: "Do you allow others to push ahead of you in line?" Of course all the Chinese said "yes," which apparently is marked on the neurotic side. Well, there are no such lines in China: they all gather around together. So everybody turned out to be neurotic.

That is only one item, of course, though there are many others

Differences on personality tests between social classes may, like the differences between Chinese and American students cited by Klineberg, be trivial. For example, a personality test for high school students contains the question, "Are you often left out of things other kids do?" The maker of the test might assume that an answer of "yes" indicates shyness and an answer of "no" indicates sociability. But it is possible that the answer is more related to social status than to sociability. Hollingshead (21) has observed that the lower-class adolescent is frequently left out of school affairs only because of his inferior status. If, therefore, there are differences between the social classes in the way they respond to personality tests, the differences may be either meaningful or trivial.

DEFINITION OF SOCIAL CLASS

The term social class as used in this paper refers to a system of socially ranked groups.² Social anthropologists who have studied various

² W. Lloyd Warner (42) made the first statement of the class-caste hypothesis, and Warner and Lunt (45) developed methods for studying the class system of a community. One should keep in mind that the class structure is not the only status control of individual behavior. As Linton (26) has pointed out, there are statuses that do not involve factors of caste and class. Sex-typing and age-grading are two examples of such status controls.

American communities in New England, in the deep South, and in the Midwest have discovered that the social conditions under which persons have access to fundamental biological and social goals are determined in many ways by a system of privilege. The people in these communities are aware of the system and recognize various social ranks. They speak of "the society people," "the solid, respectable people," "the good people, but nobody," "poor but honest folk," and "people who never amount to anything—shiftless people." In the small cities that they have studied, the social anthropologists have found three major social classes, each having two subclasses (46). The members of the community rank the three classes according to prestige as low, middle, and high, and so the social anthropologists call these classes the lower, middle, and upper classes.³

In the studies included in our review of the literature on class differences in responses to personality tests, amount of income, education, occupation, dwelling area, other similar characteristics, and various composite scales of status have been used to measure social status. Some of the composite scales (e.g., the American Home Scale) emphasize economic differentiae—income or possessions—while others (e.g., Warner's Index of Status Characteristics) emphasize social differentiae.

Warner's Index of Status Characteristics is strongly correlated with social-class placement by the evaluated participation method ($r = +.97$); thus it is evident that the ISC measures the same kind of thing that the evaluated participation method does (46, p. 183). Scales like the Kerr-Remmers American Home Scale apparently measure a related but by no means identical variable. Finch and Hoehn (15) report a product-moment correlation of $+ .71$ between the ISC and the American Home Scale. The scales that utilize social differentiae are better measures of social class as Warner uses the term and as we have chosen to define it in this paper.

Characteristics like amount of income or education are related to social-class placement but not identical with it. For example, Warner and his colleagues (46) report a correlation between amount of income and social-class placement (measured by evaluated participation method) of $+ .89$.

Although none of these measures except the ISC is substantially identical with social class as defined by Warner, they are strongly

³ A review of the literature on differences in behavior between members of the different social classes is beyond the scope of this paper. Komarovsky and Sargent (24) have briefly reviewed this literature. For detailed information the reader is referred to the studies by Davis (5, 6, 7, 8), Davis and Dollard (9), Davis, Gardner, and Gardner (10), Davis and Havighurst (11), Dollard (13), Kinsey, Pomeroy, and Martin (22), Neugarten (31), Sutherland (41), Warner, Havighurst, and Loeb (43), and Warner, Junker, and Adams (44).

enough related to social class to justify using them as rough indices of class placement. In describing each study, the present writer has carefully noted which measure of status was employed by the investigator.

STUDIES OF THE INFLUENCE OF SOCIAL CLASS ON PERSONALITY

In our survey, we have classified the various studies according to the personality test investigated. The tests are listed alphabetically; under each test, the studies are arranged chronologically.

Bernreuter Personality Inventory. Hoffeditz (20) compared the Bernreuter scores of 27 men in a low-status group, 40 in a middle-status group, and 33 in a high-status group. Her measure of social status was the Barr Occupational Scale. She found appreciable class differences on the "neurotic-tendency" and "self-sufficiency" scales and smaller differences on the "dominance" scale.⁴ An analysis of variance shows all these differences to be statistically significant at the .001 level. The lower-status subjects received the less favorable scores. Hoffeditz concluded that consistent differences were found between fathers in various occupational groups and that the highest occupational group was least neurotic, most self-sufficient, and most dominant.

Minitzer and Sargent (30) studied the relationship between family economic status and some personality traits of college students. Their measure of social status was family income, which was reported anonymously by their 120 subjects. They compared the 12 students with highest family income and the 12 with lowest family income. According to Minitzer and Sargent, "the study revealed no striking relationship between the family economic status of students and their personality traits . . . [but] economically underprivileged students tend slightly toward the maladjusted personality pattern" One should keep in mind that there were probably very few if any lower-class students in this college group (college students are predominantly middle-class). This would tend to restrict the range of the class variable and therefore to reduce the size of correlation coefficients.

Patterson (32) studied the relationship of Bernreuter scores to various characteristics in parents whose children were being studied by the Fels Research Institute. He reported the correlations between income and the various Bernreuter scales for a group of 88 men. The correlations were all low, ranging from .13 to .26. The largest correlations

⁴ Throughout this review, differences between groups are described as "appreciable" when they are at least as large as 1 P.E. (P.E. calculated from the distribution of an unselected population) and as "small" when they fall short of this criterion.

were between income and the B2-S scale (self-sufficiency), $+.26$; and between income and the B4-D scale (dominance), $+.22$. These two correlations are statistically significant at the $.05$ level.

Sumner (40) gave the Bernreuter Inventory to 205 Negro college women. Since the subjects were all college students, one may safely assume that almost all belonged to the middle class. Sumner took from the group of 205 the 51 who scored highest on the Bernreuter neurotic scale and the 51 who scored lowest on this scale and compared their scores on the Sims Score Card. The mean status score of the more neurotic group was lower than the mean status score of the less neurotic group. This difference fell short of significance at the $.05$ level ($p = .084$). Sumner concluded that no significant differences were found in socioeconomic status between the most neurotic and the least neurotic of the Negro college women studied.

Sumner's conclusion that no difference in status has been demonstrated between the most neurotic and least neurotic of Negro college women follows from his data, but this finding does not mean that there is generally no relationship between social status and personality inventory score. The college group is a sample of very restricted range in social status. Thus there is little chance for any relationship between social class and personality test score to show up.

The Brown Personality Inventory. Brown (2), in a study standardizing his personality inventory, computed the correlation between the test scores and pupils' scores on the Sims Score Card. His subjects were 441 fourth- to ninth-grade children from schools in three Ohio cities. The correlation between the Sims score and the personality test score was $-.118$ ($p < .05$). Brown concluded "... neuroticism does not predominate in any particular social or cultural group."

From this study one would be tempted to conclude that there is only a very slight relationship between personality test score and social status. One must keep in mind, however, that the measure of social status was the Sims Score Card, which emphasizes economic rather than social aspects of status; a measure stressing "social" aspects might have given a different result. Furthermore, it is not possible to tell from Brown's published article whether a wide range of social status was represented by his subjects. If the range was very restricted, no appreciable correlations could be expected.

Two years later Brown (3) published a further study of cultural influences on test responses. The subjects included 100 high-status, 100 middle-status, and 100 low-status pupils from Columbus, Ohio schools. These children were from 9 to 15 years old. Status was estimated from the type of neighborhood in which the children lived. On

all five parts of Brown's test the low-status children differed significantly at the .01 level from the middle- and high-status children. The differences between the social classes were most pronounced on the physical symptoms, insecurity, and irritability scales. These differences were, however, relatively small. Brown concluded, "It is interesting to observe that scores in general appear to bear a close relationship to the socioeconomic status of the groups" (3, p. 331).

This result seems contradictory to Brown's earlier finding. The apparent contradiction can be explained by the different kind of measure of social status (a social rather than an economic measure) and perhaps by a wider range of social status among the pupils in the later study, which thus allows any differences to be shown more strongly.

Springer (36), troubled by the apparent discrepancy between the findings of Brown's earlier study and of his later study, investigated the relationship between social status and test scores in 327 low-status boys and girls and 473 middle-status boys and girls. The subjects were 9- to 15-year-old New York City school children. Springer used two different methods of measuring social class. First, pupils were assigned to a low-status group if they lived in a poor neighborhood, and to a middle-status group if they lived in a good neighborhood. Second, pupils' fathers were rated on the Barr scale of occupational status. Springer reported significantly greater mean neurotic scores for the low-status group than for the high-status group (that is, for the children living in the poor neighborhood than for those living in the good neighborhood), but he did not find any appreciable correlation within each status group between the Brown neurotic score and the Barr rating.

Here again it is seen that one stands little chance of finding a statistically significant relationship between test score and social status if the range of social status among the subjects is restricted. The difference in mean personality test score between the lower-class group and the middle-class group studied by Springer is appreciable.

Gough (16) reported a study of the relationship between social status and scores on the Brown Personality Inventory. The subjects were 73 sixth-grade children from three public schools in a Minnesota city. The three schools were selected to represent the highest, the lowest, and a middle school on the social-status continuum. There was a small, not statistically significant difference in the personality test scores of the children in the various schools. The children in the high- and medium-status schools received more favorable scores than the children in the low-status school.

Gough also used the American Home Scale to measure social status

of the pupils. This scale puts a heavy emphasis on the economic aspects of status. He found only a slight correlation ($r = -.16$), not significant at the .05 level, between the American Home Scale and the personality inventory score. The higher-status children got more favorable scores.

California Test of Personality. Stott (39) has reported a study of 806 adolescents living in Lancaster County, Nebraska. He found that farm children got better scores on the self-adjustment part of the California Test of Personality than non-farm children ($p < .01$). Farm and non-farm children were not significantly different on the social adjustment part of the test. Among the children of non-farm families, occupational status as measured by the Minnesota occupational classification did not seem to be strongly associated with personality test score. The scores of occupational class VII, the lowest occupational group, were, however, somewhat lower than the scores of the other groups ($p < .20$).

In another paper Stott (38) reported correlations between the California Test of Personality and both the economic- and cultural-status level of the family and the "farm class" of the family (i.e., how good a farm the family owned or rented). The product-moment correlation between the self-adjustment part of the test and economic level of family was $+.14$ ($p < .05$). Between self-adjustment and cultural level, $r = +.20$ ($p < .01$). Between self-adjustment and farm class, $C = +.31$ ($N = 183$). The correlations between social adjustment and economic level or cultural level were not significant. The coefficient of contingency between social adjustment and farm class was $+.30$ ($N = 183$).

One may conclude that, with the measures of social status used by Stott, only a slight relationship was found in the group studied between scores on the California Test of Personality and social status. The self-adjustment part of the test showed a greater social-class differential than the social-adjustment part. The higher-status children got the more favorable scores on the test.

Minnesota Multiphasic Personality Inventory. Gough (17) gave the MMPI to 223 high school seniors (90 boys, 133 girls) from a Minnesota city of 25,000. He used the Sims Score Card as a measure of social status, taking the top 38 and bottom 38 as measured by score-card ratings as his high- and low-status groups. Gough found 34 items out of 550 on the MMPI with differences significant at the .02 level. The differentiating items were grouped by Gough into five classes: (a) positive literary and esthetic attitudes, (b) social poise and self-confidence, (c) denial of fears and anxieties, (d) broad-minded, frank attitudes toward moral, religious, and sexual matters, and (e) dogmatic, self-righteous opinions.

The descriptions apply to the choices of the higher-status group. There were no statistically significant differences on any of the MMPI scales (taking each scale as a whole) between the high-status and the low-status groups.

One must keep in mind, of course, that there were only 38 subjects in each group. The slight differences that did appear in Gough's analysis showed the high-status group with more favorable scores.

When Gough (18), in a second part of this study, correlated the Sims score with the various MMPI scales, he found five scales with correlations significant at the .05 level: the *K*, *F*, *Hs*, *Pd*, and *Sc* scales. Correlations of all of these scales with social status were low, ranging from $-.13$ to $-.21$. In each instance the low-status group received the more unfavorable score. Gough concluded that the high-status pupils showed more defensiveness and reserve in regard to personal affairs and problems (*K* scale), greater conventionality (*F* scale), fewer somatic complaints (*Hs*), more satisfactory over-all adjustment (*D*), less-tendency toward serious personality disturbances (*Pd*, *Sc*). As we have pointed out, these correlations are all quite low.

From Gough's description of his subjects, it seems likely that most of them belong to the middle class and very few to the lower class. In view of this restriction in the range of social-status scores, it is not surprising that Gough found no very high correlations between social status and the MMPI. He did, however, discover 34 items with a statistically significant social-class differential, and he combined these into a special MMPI scale that he calls the status (*St*) scale.

Minnesota Scale for the Survey of Opinions. Menzies (29) studied the relationship between responses to the Minnesota Scale (short form) and various social characteristics. His subjects were 177 men and women from 15 to 29 years old who either belonged to the so-called "cellar clubs" in a medium-sized New York city or worked for the NYA. He found in general that the poorer subjects had lower morale scores and less favorable adjustment scores. For example, the respondents whose families were on relief had a poorer adjustment score than respondents whose families were not on relief, and the difference between these two groups was appreciable. Similar differences were found between those whose fathers were unemployed and those whose fathers were employed, between the NYA workers and privately employed workers, and between NYA workers and students.

The group studied by Menzies is a rather special group, and it is hard to know exactly what population his results can be generalized to. Apparently the results of his study give some slight support to the gen-

eralization that people rating lower in social status generally receive poorer scores on personality tests.

Davidson (4), in a study of the relationship between personality and income in 102 highly intelligent 9- to 13-year-old children, found no statistically significant relationship between income and Minnesota Scale family, morale, or inferiority scores.

Pintner Aspects of Personality Inventory and Pressey X-O Test. Maddy (28), in a study of the relationship between personality and parental occupation, tested 319 sixth-grade children in Washington, D. C., whose fathers were professional men or semiskilled workers. The children whose parents were professional men got more favorable scores on the Pintner Aspects of Personality Inventory. The difference, although statistically significant, was small.

The middle-class children reported fewer worries on the Pressey X-O Test. Thirty-five was the average number of worries for the middle-class child compared to fifty for the upper-lower-class child, an appreciable difference. There were no significant differences on the "disapproval" or "interests" sections of this test. "Idiosyncracies" scores were also about the same in both groups.

Maddy concluded that there were reliable differences in personality traits between the middle-class and the upper-lower-class children. She found that the differences were greater among the girls than among the boys. Although the differences found by Maddy are statistically significant, they are relatively slight except for the "worries" section of the Pressey X-O Test.

Rorschach Method. As part of a comprehensive study of the relationship between personality and economic background, Davidson (4) gave the Rorschach test to 102 9- to 13-year-old boys and girls: 49 bright and talented children attending a New York City public school and 53 attending a New York City private school. Her subjects were almost wholly middle class, since both the public school and the private school attracted mainly middle-class children. That the children were exceptional is shown by their unusually high intelligence: the average IQ was 143.

Davidson concluded that "income is not a significant source of variation either in influencing personality pattern, or in determining the degree of adjustment or in establishing specific personality characteristics" (4, p. 108). However, some scores were correlated with income. Children of higher-income families gave more *CF* and *C* responses, got a higher sum-*C* score, and gave fewer "popular" responses than children of lower-income families.

The results of this study should not be generalized beyond the kind of group studied by Davidson, namely, a group of highly intelligent, mainly middle-class children.

Schachtel and Levi (34) studied a group of 50 children in a wartime day nursery and a similar group in a private nursery school, the children of prosperous middle-class parents. They reported that a larger proportion of the day-nursery (lower-class) children were "not loved" than of the nursery-school (middle-class) children, and that a larger proportion of the nursery-school group were "pseudo-loved" than of the day-nursery group. These authors do not indicate on what basis they decided to label the children's Rorschach responses as indicative of a "loved," a "not loved," or a "pseudo-loved" child. Since Schachtel and Levi do not report exactly how they carried out their study, we do not know how to evaluate it as a scientific report.

Auld (1) has reported a comparison of the responses of lower-class and middle-class adolescents. He used data gathered by Hertz (19) on 41 upper-middle-class boys and by the Gluecks (35) on 500 lower-class nondelinquent boys. The boys studied by Hertz were 15 years old at the time of testing; those studied by the Gluecks ranged from 9 to 17 years old ($\bar{X} = 14.6$).

Auld reported differences between the number of human-movement responses, animal-movement responses, form-color responses, color-form responses, and color responses given by the two groups. The differences in number of *M* responses, *FC* responses, *CF* responses, and total number of color responses were all significant when tested by the chi-square test ($p < .01$). The difference in the number of pure color (*C*) responses was not statistically significant, since few subjects in either group gave any of these responses. The difference in the average number of animal-movement responses given by the two groups may be statistically significant, but the sources from which the data are drawn do not give sufficiently detailed information to permit chi square to be used to test the reliability of the difference. (A *t* test is inappropriate because of skewness of the distributions.)

These differences cannot be accounted for by the difference in number of responses between the two groups. The mean number of responses for the middle-class group is 31, for the lower-class group, 27. If movement and color responses were figured as percentages of total responses, rather than as absolute numbers, the middle-class group would still come out higher on movement and color responses.

A study such as this, utilizing whatever data happen to be at hand, has the disadvantage that the groups may differ on other variables

than social class. Furthermore, there may be some differences between the groups on account of differences in methods of administering the test. The findings of Auld should, therefore, be accepted only tentatively, as suggestive of the kind of differences that may be found in more carefully controlled studies.

Schachtel, in an appendix to the book by Glueck and Glueck (35), has reported differences similar to those reported by Auld between the responses of lower-class and middle-class adolescents to the Rorschach test. He states, "The Rorschach records in the present study tend to be more meager and less expressive than those of the metropolitan middle class. As far as we can see, this is not a function of differences in age, but of the difference in social stratum" (35, p. 363).

SRA Youth Inventory. Auld (1) has reported that there are 84 items out of the 298 on the SRA Youth Inventory on which low-income and high-income pupils respond differently. Auld utilized data presented by Remmers and Shimberg (33) in the manual for the test, using a breakdown of the responses of 2500 high school pupils in the group on which the test was standardized. The subjects had been assigned by Remmers and Shimberg to a high or a low economic group on the basis of a short questionnaire asking them whether their family had a vacuum cleaner, an electric or gas refrigerator, a bathtub or shower, a telephone, and an automobile, whether they had paid lessons in dancing, art, music, and the like, and whether their father had finished high school. This short questionnaire obviously emphasizes economic aspects of status. Auld had assumed, however, that there would be more middle-class pupils in the high group than in the low group and more lower-class children in the low group than in the high group. He found 84 items (out of 298 in the test) that showed a social class differential significant at the .01 level of confidence (by pure chance one would get only 3 items out of 298 statistically significant at the .01 level). Yet the mean scores of the low and the high groups differed only slightly.

The differences found were not confined to questions that, on the face of it, were more "social" than "personal." Many of the items that the authors of the test believed indicate basic personality difficulties showed differences between the two groups. Of 101 "basic difficulty" items, 22 showed a social-class differential.

The lower-class group checked more items on seven of the eight subscales of the test. The middle-class group checked more in the eighth area. Since the checks are considered indicative of maladjustment, the lower-class group got a less favorable total score on the test.

Thematic Apperception Test (Thompson Modification). Korchin,

Mitchell, and Meltzoff (25) reported that middle-class subjects gave longer stories on the TAT than lower-class subjects ($p < .01$). Their subjects were 80 middle-class and 80 lower-class men and women (20-40 years old). Half of each group were Negro and half were white.

Vineland Social Maturity Scale. Louttit and Watson (27) administered the Vineland scale to 120 entering first-grade children who acted as their own informants. Social class was measured by the kind of school the child attended (in a poor, medium, or good area) and by the father's occupation. Children attending the better schools and children whose fathers were in higher-status occupations got appreciably better social maturity scores. The authors concluded that social maturity scores are probably affected by the socioeconomic cultures from which the children come or by the parental intelligence associated with the socioeconomic culture.

Wisconsin Scale of Personality Traits. Stagner, after studying the test scores of 128 college students, concluded that "there is evidence that children reared in homes which are lower economically . . . tend to develop traits of nervousness or emotionality, introversion, inferiority feeling and social passivity or seclusiveness" (37, p. 552). The differences between the various economic groups were, however, small. Only on the *W* scale (emotional instability) was the difference between the two extreme groups as large as 20 percentile points. The college group is, of course, one with a very restricted range of social status. Stagner did not compute standard deviations or critical ratios for the differences. In this study, economic status was measured by students' self-ratings on a five-point scale.

Woodworth-Matthews Personal Data Sheet. Wrightstone (47) gave this test to 100 pupils in the fifth to ninth grades of the Summit, New Jersey schools. He used the Sims scale as a measure of social status. He reported a correlation of $-.52$ between the Sims scale and the Woodworth-Matthews score, indicating that higher social status was associated with more favorable scores on "emotional stability."

CONCLUSIONS

This paper has reviewed the studies on social-class differences in responses to personality tests. Most of the studies included in this survey show social-class differences on the tests investigated. In about a third of the studies these differences are appreciable. The differences reported are generally larger when "social" rather than "economic" measures of social status are used and when the subjects have a considerable spread in social status. When an economic measure of status

is used or the group studied has a restricted range of social status, the investigators have usually reported only slight relationships or none at all between social status and the test score. On some tests the differences between classes are appreciable, on other tests the differences are quite small. Even when the differences in total score are relatively slight, however, there are often a considerable number of test items that show a marked social-class differential.

On the basis of the studies reviewed, it appears that middle-class and lower-class subjects differ in their responses to some personality questionnaires and to the Rorschach test and Thematic Apperception Test. In every study that showed differences between classes, the middle-class subjects got more favorable scores than the lower-class subjects. The evidence presented here, however, does not indicate whether these differences are meaningful or trivial.

Because members of different social classes may differ in their responses to personality tests, it is necessary to take these differences into account whenever we make use of personality tests in research or in clinical practice. If the psychologist does not explicitly take account of these differences he is likely to assume a bias in favor of his own social class, the middle class. For example, he may use norms derived from middle-class groups, like the Hertz norms for the Rorschach, or he may judge the meaning of the items in a personality questionnaire from his middle-class point of view. To avoid these errors the psychologist must consciously take social-class differences into account.

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THE EVALUATION OF EXPERIMENTALLY CONTROLLED CRITERIA¹

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Many of the most relevant objective criteria of performance in military and industrial situations are rendered comparatively useless because of their unreliability. Usually they are complex, and their variance is determined by a multitude of factors other than the performance of the individuals or groups being evaluated. As a result, correlations obtained with these criteria are low and efforts to improve upon them are often frustrating. It is the purpose of this paper to consider some of the problems of estimating or predicting the improvement which might be expected to result from the exercise of experimental control over selected sources of extraneous variation within such a criterion.

As an example of a complex and unreliable criterion, and one on which much effort has been expended, consider the average error of a bomb drop, computed as a radial error (so-called circular error) from the point of impact of the bomb to the aiming point. On the face of it, this measure is an ideal indicator of the ability of a bombardier to drop bombs. Experience, however, shows that bombardier variance makes up a relatively insignificant portion of the total observed variance of the measures, for such factors as altitude and air speed of the aircraft, turbulence of the air, abilities of the pilot to hold a course, as well as equipment variations, contribute to the magnitude of the single score.

Many efforts have been made to improve circular error measures by means of statistical and experimental controls (10). A typical statistical method involves the computation of average standard scores for individuals, each standard score indicating the relative position of the individual in the distribution of the dependent variable for a single extraneous variable category. A similar procedure utilizes conversion factors, usually multiplicative constants determined empirically from the circular error distribution of each extraneous variable. A third method, one of experimental control rather than statistical correction, consists of criterion or test missions in which the operation of the extraneous variables is controlled either by excluding the variable (holding it constant) or randomizing its effects with reference to individual bombardiers. It is the last method with which this report is concerned.

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In situations such as this (as well as in comparable laboratory studies²) much time and effort can be saved through the use of appropriate estimates of the increased precision to be achieved by experimental control. Frequently a meaningful estimate can be made from a pilot study designed to predict the feasibility of experimental control. Two types of estimate are considered here. One is an estimate of what the reliability (proportion of true variance) of the criterion variable would be if certain of the extraneous variables were perfectly controlled in the sense of having all of the variance removed. The second is an empirical estimate of the minimum variance of the extraneous variables which might be expected under specifically defined operational conditions.

Of these, the first permits an estimate of an ideal state in which the criterion has been corrected for the variation contributed by noncriterion sources. The second yields a practical estimate of the proportion of the obtained criterion variance which is available for prediction (correlation) purposes. Both types of estimate are simple in principle and can be achieved in several ways—through correlation analysis or by modification of analysis of variance techniques. The latter method will be used with this example.

In the simplest case of bombing error determined by only two variables, the bombardier and equipment differences, one might expect to obtain data similar to those diagrammed in Figure 1. There the hypothetical errors of 100 bombardiers flying with four different sets of equipment are tabulated. The total variance in this table may be attributed to (a) equipment variation, (b) differences among individuals, and (c) inconsistencies in individual's relative position on the error scale among equipment classes.³ If each measurement of each individual with each equipment were perfectly consistent, the individual rank orders would be the same in each equipment subdivision and (assuming equal within-equipment distributions⁴) the rank differences (in circular error units) would be equal from subject to subject and from equipment to equip-

² The estimates involved are typical of laboratory measurement situations where the behavior of the dependent variable is complexly determined and it is desired to observe the effects of only one or two independent variables.

³ This is to a degree a simplification, for some sources of error which could be classified and for which magnitudes could be estimated (i.e., interaction effects) have been left in the "unreliability" category.

⁴ This assumption often cannot be met, for it is the nature of many extraneous sources of error to affect the variability within classes as well as the average values of the class. An example of such a variable with circular error is the bombing altitude. As altitude increases the mean error increases as does also the variance of the "within-altitudes" distribution.

ment. This would be the same as saying that the measurements of circular error are perfectly reliable, ignoring the variance determined by equipment and assuming no other unidentified factors present.

The approach to such an estimate of reliability (independent of equipment effects) may be visualized in a different way. That is to "correct" for the variation determined by equipment differences. Assuming homogeneity of variance in equipment subgroups, this can be accomplished by adding to all measures in a given subdistribution the

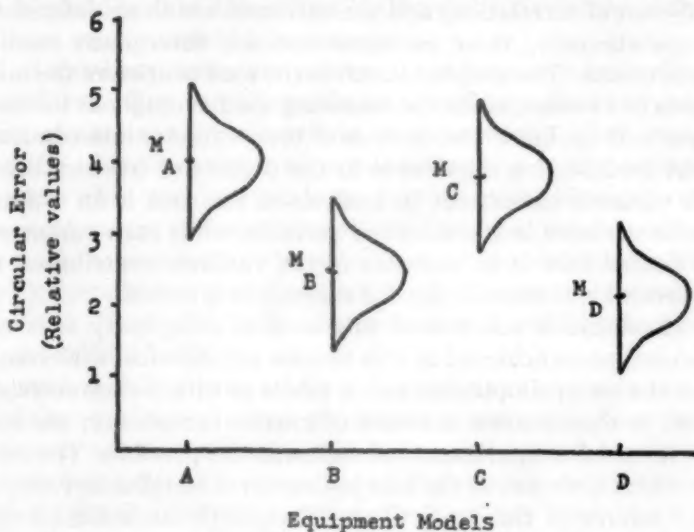


FIGURE 1. FREQUENCY DISTRIBUTIONS OF CIRCULAR ERROR SCORES FOR FOUR EQUIPMENT SUBGROUPS. (Hypothetical data; same 100 individuals on all equipments.)

difference between the mean of that distribution and some reference point on the scale (say the mean of the middle or typical equipment distribution). This reduces the variability between equipment classes to zero and leaves only variability determined by "true" individual differences and "error" or unreliability.

The second "correction" can then be introduced into the data by removing individual differences by adding a constant to all scores in order to make individual means equal. To assume that such a process of removing individual differences by a correction based on mean differences removes individual contributions to total variability requires an assumption of homogeneity of variance within individuals—a hard assumption in some situations to satisfy. If, however, this assumption can be met (as it can in this example) and there is zero correlation of within

persons variations and within machines variations, the variance remaining within the table after such a double correction is that which best indicates the "unreliability" of the measurements of the dependent variable. It may be used in the conventional reliability coefficient equation, in which r_{11} is a reliability coefficient (the equivalent of the square of the

$$r_{11} = 1 - \frac{\sigma^2 \text{ error}}{\sigma^2 \text{ total}} \quad [1]$$

eta coefficient of correlation) and the variances are those defined above.

Computationally, these estimates can be determined readily by numerous means. The simplest involves the modification of the methods of analysis of variance, since the reasoning used throughout follows that technique as it applies to the analysis of total variance into components. The chief modification required is in the degrees of freedom involved; for each variance component in analysis of variance is an estimate of population variance in the observed variable, while each variance component desired here is an estimate of the variance contributed to the total observational situation by the variable in question.

For classification schemes of this level of complexity (one control variable) estimates achieved in this fashion are identical with results secured by the use of simple conversion tables or within-class correlations. However, in classification schemes of greater complexity, particularly those of factorial design, improved estimates are possible. The estimate of error which is chosen as the best indication of unreliability may differ with the nature of the predictions. Frequently an estimate derived from an interaction sum of squares provides an appropriate indication of the likely precision in a given prediction situation.

An example of this latter type arose in attempting to make group training predictions from "learning curves" of circular error performance (4). There learning was, of necessity, inferred from a temporal (day-by-day or mission-by-mission) change in criterial (group average circular error) performance. During the observation of extraneous variables influencing criterial scores it became apparent that some of these also showed a temporal course.⁵ They produced psuedo-learning effects. A test of whether this psuedo-learning effect is sufficient to mask "true" learning effects can be made by an analysis of variance significance test of the day-to-day variance over the interaction variance of equipment and days. If an estimate is then desired of the extent to which this

⁵ This is common, for example, with equipment variations. Frequently equipment breakdown is preceded by periods of gradual change in machine function which appear in training records as "learning" variance.

interaction of equipment and "learning" variables influences learning predictions, the interaction sum of squares can be appropriately weighted to indicate the proportion of the total variance which is involved in this interaction "unreliability."

It becomes apparent at this point that the specification of the variance designated σ^2 total in formula [1] is also appropriately different in different contexts. If, in the example cited, the desire had been to make individual rather than group predictions, an appropriate total variance estimate would be that determined by the variability of the individual's means—an among-individuals variance.

All of these estimates are subject to at least two general types of disadvantages. For one thing, the assumptions underlying such subdividing of variance are sometimes hard to meet. They include the assumption of independence of subclassifications or the estimation of covariances when such independence does not exist. Similarly, the independence of means and variance is required—or, more specifically, it needs to be assumed that the subclasses of the extraneous variables for which correction is being made differ in no essential aspects except their means. This is typically approached by a test of homogeneity of variance within subclasses.

In passing, it is worthy of note that these estimates do not require some of the usual assumptions of analysis of variance. Randomness of subgroups is not essential except in so far as the requirements of independence of variables is concerned, and the requirement of normality of population distribution which is basic to the F test is not involved here. In these respects the similarity of this reliability estimate to the eta correlation coefficient is greater than it is to the population variance estimate required for analysis of variance significance tests. The correlation of such a "corrected" variable with a second variable (say a test) is thus still a "within classes" correlation.

Another weakness in some estimates arises from the fact that they do not enable one to define the extended "practical" range of prediction (validity) coefficients made possible by the experimental controls, although they do indicate the maximum theoretical or ideal values which would occur if perfect control were achieved. Since perfect experimental control is seldom reached, it is essential in any given situation to compute other estimates designed to take into account the variability of the extraneous variable which remains after experimental controls have been instituted. Such an estimate requires either a series of pilot study checks to determine the effectiveness of controls, or some information (estimated from control charts and the like) of the extent to which con-

trols are being achieved. Running estimates of reliability based on the same reasoning as the above can be used to compare the "achieved" reliability with the "ideal." The difference between the two estimates needed is essentially that the variance of the extraneous variable is excluded in one, while in the other it is left in and estimated from control charts or pilot analyses.

In conclusion, it will be noted that all of the foregoing situations are simply specific applications of the more general classes of reliability estimates in common use. Specifically, they represent application of the general formula proposed by Horst (7) and applied by him to test situations. The logic is essentially that of test-retest measurement situations as outlined by Guttman (6), Alexander (1), Cronbach (3), and Jackson (9). It has also been applied to laboratory measurement situations by Melton (11, 12) and Chapanis (2).

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RELIABLE AND QUESTIONABLE SIGNIFICANCE IN A SERIES OF STATISTICAL TESTS¹

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Not infrequently the psychologist has to carry out a large number of tests of significance. Analysis of items constituting a questionnaire may serve as an example.

A psychosomatic inventory² was administered to a group of normotensive and hypertensive individuals (systolic blood pressure of 140 mm. Hg and over, or diastolic blood pressure of 90 mm. Hg and over). The inventory consisted of a series of 228 items focussed on a variety of personality aspects described in the literature as being characteristic of individuals who are prone to develop high blood pressure. Each item was tested for significance of the difference between the percentage of "Yes" answers given by each of the two groups. With the 5 per cent level of the critical ratio ($C.R. \geq 1.96$) as the criterion of significance, 24 items showed differences significant at this level or better.

Wilkinson (2) pointed out the necessity, under conditions of repeated tests of significance, for determining whether the number of "significant" items is sufficiently great to indicate a nonchance occurrence and provided a table giving the probability of obtaining n or more significant statistics by chance in a set of N tests. Values were given for N from 1 to 25, n from 1 to 4 for statistics significant at the 1 per cent level and n from 1 to 7 for the 5 per cent level of significance. The tables are useful but the N is too small for most item analyses. It is the purpose of this note to point out an approximate but satisfactory solution for conditions in which N is large.

Our task, to rephrase the problem, is to determine the probability of a chance occurrence of the compound event of obtaining n significant critical ratios ($n = 24$) in a series of N tests of significance ($N = 228$), the constituent events being independent. Each event has only two possible outcomes, i.e., the critical ratio is either significant or nonsignificant. The probability of chance occurrence of a significant difference at a single trial was set, by the criterion for statistical significance, at 5 per

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² LABORATORY OF PHYSIOLOGICAL HYGIENE, UNIVERSITY OF MINNESOTA. Personality differences between normotensive and moderately hypertensive individuals. II. Results obtained from a psychosomatic inventory. (To be published.)

cent ($p=0.05$); the probability of nonchance occurrence is 95 per cent ($q=1-p=0.95$); $p+q=1.00$.

For repeated trials the probabilities of the occurrence of "significant results" by chance may be determined by the expansion of the binomial $(p+q)^N$. For two trials we have

$$(p+q)^2 = p^2 + 2pq + q^2.$$

Substituting for p and q , we obtain $0.0025+0.0950+0.9025$, where the first term indicates the probability of obtaining by chance two significant results in the two trials, the second term refers to the probability of obtaining by chance one significant result, the third term gives the probability of obtaining no significant result by chance (probability of obtaining of a significant result through other factors than chance alone). As a rule the first two terms would be summated, yielding 0.0975 , which indicates the probability of obtaining one or more significant results by chance in two trials. For three trials we have

$$(p+q)^3 = p^3 + 3p^2q + 3pq^2 + q^3.$$

The binomial expansion yields the probabilities of the chance occurrence of 3, 2, 1, and 0 significant results. Again we would summate to get the probabilities of obtaining one or more and two or more such results. It is apparent that with an increase in N the computational labor quickly becomes unmanageable.

The probability of obtaining by chance n or more critical ratios in series of N tests may be obtained exactly from the equation

$$\begin{aligned} P &= \sum_{x=n}^N \binom{N}{x} p^x q^{N-x} \\ &= 1 - \sum_{x=0}^{n-1} \binom{N}{x} p^x q^{N-x}, \end{aligned}$$

where $x=0, 1, 2, \dots, n-1$. Then, in our specific case,

$$P = 1 - \sum_{x=0}^{23} \binom{228}{x} (.05)^x (.95)^{228-x}.$$

It is obvious that the direct calculation of the P value is hopelessly arduous.

Given a large N and $Np \geq 5$ (with a $p \leq 0.5$), the binomial distribution can be approximated satisfactorily by the normal distribution with mean $m = Np$ and standard deviation $\sigma = \sqrt{Npq}$ (1, p. 41). To solve our

problem we compute a critical ratio, $C.R. = (n - m)/\sigma$ and determine from the available tables the area to the right of the obtained $C.R.$ value.

For our case $m = 228 \times 0.05 = 11.40$,

$$\sigma = \sqrt{228(0.05)(0.95)} = \sqrt{10.8300} = 3.29;$$

$$C.R. = (24 - 11.40)/3.29 = 3.83,$$

yielding a negligible probability ($P = 0.00006$) of obtaining 24 or more significant critical ratios in a series of 228 items tested for significance of the difference. The normotensive and the hypertensive subjects differ reliably in their responses to a psychosomatic inventory.

The binomial frequency distribution is discrete, not continuous. In carrying out the tests based on normal distribution, which is continuous, we may consider frequency 24 as occupying the interval from 23.5 to 24.5. It is more exact to calculate the area under the normal distribution to the right of the value 23.5.

It is hoped that this note will be found useful by others who are confronted with similar problems, frequently emerging when a large number of statistics is calculated. Strangely enough, the matter has not been adequately and specifically discussed in the standard texts of statistics which are likely to be consulted by psychologists.

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THE EFFECTIVENESS OF SHORT METHODS FOR CALCULATING CORRELATION COEFFICIENTS

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Two factors in recent years have emphasized the value of short methods for calculating correlation coefficients. One of these is the tendency to use larger numbers of variables. If there are 5 variables for which intercorrelations are to be obtained, the number of correlations to be calculated is 10. If there are 10 variables this number jumps to 45. If the number of variables is 20 there will be 190 intercorrelations. And if the number of variables is 100, the number of correlations to be calculated is 4950. Thus, unless electrical or mechanical equipment is available the large numbers introduce serious problems. Even if computing equipment and trained personnel are available the costs make shortcuts desirable. The second factor is the realization that large samples are essential if stable findings are to be hoped for in correlational studies. If the problem is to establish that a relationship of useful size does or does not exist between two variables, the sample may not have to be very large. To obtain predictive weights as by multiple correlation procedures, several hundred, if not several thousand, cases are likely to be necessary. For factorial analysis procedures large samples are similarly essential.

For these reasons short methods have become increasingly desirable but data on the comparative accuracy of the various short procedures have not been available. It is the purpose of this paper to discuss, first, some of the principles to be used in selecting a suitable short method for a particular problem; second, to present some data on the time required to compute coefficients by the various methods; and third, to present data regarding the relative accuracy of these procedures as compared with the longer product-moment computational methods.

TYPES OF SHORT METHODS

Turning first to the considerations involved in developing or selecting a short computational method it is apparent that shortcuts can be obtained in four ways. First, it may be possible to cut the number of operations that have to be performed on the data; second, time may be saved by substituting simple numbers for larger ones; third, simple

operations such as looking up a value in a table may be substituted for more complex operations; and fourth, the operations can be performed only on selected data or only once for a group of data. In other words, you can cut the number of things you have to do, make it easier to do these things, and cut the number of times you have to do them.

The type of equipment available to assist will frequently be an important consideration. Methods which seemed efficient and desirable for hand computations became obsolete and inefficient with the introduction of modern calculating machines. The graphic item counter of the IBM test scoring machine, various types of punched card, magnetic, and electronic procedures for recording and operating on numbers play an important role in most current computational procedures.

However, in many studies shortcut procedures which use no equipment except pencil and paper are very satisfactory, and actually faster and more efficient in some cases than procedures using the elaborate and awe-inspiring new types of machines.

To return to the problem of shortcuts for correlation computation procedures, the first thing suggested was to cut the number of operations to be performed. If, in some way, the data can be gotten into standard score form with means of zero and standard deviations of one, the computations involve only the summing of the multiplied pairs of values and dividing by their number. The second principle is to make it easier to do these operations. This suggests using simple numbers such as 1, 2, $\frac{1}{2}$, and 0. The third principle suggests finding simpler operations to perform. Counting the number of cases falling above or below some fixed value and comparing these numbers with tabled values of those that would be found in correlation surfaces having known coefficients appears much simpler than multiplying, summing, and dividing. The fourth principle involves cutting the number of times each operation is to be performed. This suggests grouping. Two groups might be used but it is found more efficient to use three, five, seven, or some odd number so that the middle group has a mean standard score value of zero.

These considerations have led to the development of two basic types of techniques. One involves the use of coarsely grouped standard scores so that the numbers multiplied are simple, such as 2, 1, $\frac{1}{2}$, 0, $-\frac{1}{2}$, -1, and -2 (5, 11). The other type of technique involves counting the cases in a specified tail of one of the distributions which exceed a specified value on the other distribution (2, 4, 5). This value and the corresponding value at the other end of the distribution are used to enter a two-way table to find the value of the correlation coefficient in a surface which showed these proportions in this part of the area.

Some years ago Kelley (8) showed that if upper and lower groups are to be used in which the cases are to be counted, the optimal size groups for discriminating between cases at the two ends of the distribution are 27 per cent. The present author prepared tables for obtaining correlations for such groups in 1936. A chart for the same purpose was published in 1939 (3).

One of the disadvantages of the procedure is that the extreme cases at the two ends of the distribution are grouped in with others closer to the mean and near the first and third quartile points respectively and do not receive the greater weight they deserve. To remedy this the present author has recommended (1) that the values nearer the extremes be counted twice to give them double weight in determining the appropriate proportions.

In the fall of 1950 it seemed that it would be desirable to get an accurate determination of the optimal proportions to be used if the extreme categories were each to receive double weight as compared with the cases in the next category. The most suitable procedure appeared to be to obtain those five categories which would maximize the proportion of the variance in the between groups variance as compared with the within group variance. By a series of successive approximations it was found that this was achieved by using groups of 9, 20, 42, 20, and 9 per cent. The cases in the extreme groups of 9 per cent are each counted as two in adding them to those in the next groups of 20. The middle group of 42 per cent is ignored.

Tables have been prepared giving the correlation coefficients which would have various proportions in the upper and lower 29 per cent of the distribution when the extreme 9 per cent is given double weight.

Similarly, it has been determined that if the most extreme cases are to be given triple weight and the next double, as compared to the larger groups, the maximum between groups variance will be obtained by using groups of 4, 8, 25, 26, 25, 8, and 4 per cent respectively.

It is of considerable interest to note just what proportion of the variance is between groups variance in each of the three cases described above. For upper-lower 27 per cent the value is 81.0 per cent. This value was found to be the maximum for equally weighted upper-lower groups, confirming Kelley's previous finding. For upper-lower 9, 20, the value is 90.9 per cent. For upper-lower 4, 8, 25, the value is 94.6 per cent. Thus it appears that only a small portion of the available information is lost by this type of grouping. It is interesting to note that the double weighting of the top 9 per cent regains more than half of the variance lost by using only the single upper and lower 27 per cent groups.

Tables have been prepared by the author for each of the above conditions.

When the data are on answer sheets and the test scoring machine can be used, these procedures are very efficient. In some instances where the data are in different form the use of coarsely grouped standard scores is desirable. Using the Kelley Statistical Tables, it is found that the upper 6 per cent of a normal distribution has a mean deviation of 1.985. The next 21 per cent has a mean deviation of 1.008. The next 8 per cent has a mean deviation of 0.497. The middle 30 per cent has a mean deviation which, of course, is zero. These coarse groups have a between groups variance of 93.9 per cent of the total variance. In the spring of 1950 tables were prepared for using this method to compute biserial correlation coefficients in connection with two projects with which the present author was connected. One of these (11) was a study for the United States Civil Service Commission on which the author was consultant to personnel of the Cincinnati Regional Office. The other (9) was a report prepared for the Personnel Research Section of the Adjutant General's Office, Department of the Army, by the department of psychology, University of Pittsburgh. The table used in these studies was copyrighted by the author and published by the American Institute for Research in September 1950 (5). A graph which can be used for the same purpose was prepared by personnel of the Test Development Section, United States Civil Service Commission, and published by *Psychometrika* in June 1951 (6).

At a cost of only a small portion of the available information (4 per cent of the total variance) the two 8 per cent groups may be added to the zero group. This group will contain 46 per cent of the cases and the computations will be appreciably simplified because of the increase in the number of zero crossproducts and the elimination of fractions.

EFFICIENCY OF SHORT METHODS

Data are available regarding the time required by these procedures. For product-moment coefficients using coarsely grouped data in these seven categories Zagorski (12) and Hahn (7) computed four or five hundred coefficients based on about 100 cases each. The time required averaged about five minutes per coefficient including all operations. It is of special interest to note that these operations were all performed using only pencil and paper and without the aid of mechanical or electrical computing machines.

Under Lange's supervision college students computed 8,000 biserial

correlation coefficients using the coarsely grouped normalized standard scores for samples of two to three hundred cases (9). These coefficients required about two minutes each including all necessary computations.

Several thousand coefficients have been computed under the author's supervision using his upper-lower groups tables. These require less than one minute each including all operations with the counting done on the graphic item counter of a test scoring machine.

It seems safe to conclude that these times represent costs substantially below those usually incurred in calculating correlation coefficients. Under some circumstances the form of the data or other conditions may make other procedures more efficient. However, for a wide variety of situations these shortcut methods seem to offer substantial savings.

ACCURACY OF SHORT METHODS

The question remaining is: "How accurate are they?" Data bearing on this point were collected by means of an empirical sampling study on which the author was assisted in the early phases by Miss Leone Cheshire.

Populations of 10,000 cases each in which the correlations were .00, .45, and .90 were set up with the help of Pearson's tables (10). Each case was given a number and 100 samples of 100 cases each for the three populations were drawn by using tables of random numbers including Tippett's tables and a set of tables kindly supplied by Dr. Joseph Berkson.

The coefficients were then obtained using a number of computational procedures. The results are shown in the Table 1. It is seen that the means of the 100 values are in all cases fairly close to the population values. On the whole, the tetrachoric coefficients show the greatest discrepancies. The most crucial values to be observed in the table are those in the three columns headed σ_a . These represent the actual standard deviations of the distributions of 100 coefficients as computed for each of the populations by the various methods. The tetrachoric coefficients are the least accurate in all cases. For the population in which the correlation is .90, the method of upper-lower 16 per cent shows little variability when there are only 100 cases, because in so many instances all of the cases in the upper 16 per cent are in the success group and vice versa for the lower 16 per cent.

The standard error for the upper-lower 27 per cent procedure compares very favorably with biserial r . Biserial r obtained from normalized coarsely grouped scores actually is smaller than the biserial r calculated by the more conventional procedures. The differences are very slight and are likely to represent the operation of chance factors which would

TABLE 1

EMPIRICALLY OBTAINED SAMPLING ERRORS OF CORRELATION COEFFICIENTS
COMPUTED BY VARIOUS METHODS

Computational Procedure	100 Samples of 100 Cases Each From A Population in Which $r = .00$				100 Samples of 100 Cases Each From A Population in Which $r = .45$				100 Samples of 100 Cases Each From A Population in Which $r = .90$			
	M_r	σ_a	σ_t	SE_σ	M_r	σ_a	σ_t	SE_σ	M_r	σ_a	σ_t	SE_σ
Product-Moment	-.006	.086	.101	.007	.455	.078	.081	.006	.897	.020	.019	.001
Biserial r	-.007	.108	.125	.009	.444	.117	.106	.008	.903	.043	.044	.003
Biserial r (Normalized Groups)	-.008	.105	.125	.009	.449	.115	.104	.007	.911	.040	.043	.003
$r_{U4.8,25L4.8,25}$	-.008	.111	—	.008	.450	.109	—	.008	.896	.037	—	.003
$r_{U9.20L9.20}$	-.012	.113	—	.008	.448	.115	—	.008	.893	.036	—	.003
r_{U17L17}	-.005	.131	—	.009	.452	.119	—	.008	.892	.037	—	.003
r_{U18L18}	.002	.123	—	.009	.461	.132	—	.009	.880	.021	—	.002
"tetr."	.021	.186	.157	.013	.436	.149	.134	.010	.889	.055	.048	.004

M_r = mean value of 100 coefficients.

σ_a = standard deviation of 100 coefficients.

σ_t = standard error of coefficient from formula.

SE_σ = standard error of the standard deviation for these samples.

be unlikely to be reproduced in another set of samples. For those coefficients for which it has been possible to derive sampling error formulas, the observed values agree fairly well with the theoretical values.

In conclusion it may be said that these data suggest that in many situations it would be desirable (a) to plan the study so as to include a few more cases than would be necessary to obtain the desired degree of sampling stability, and (b) to take advantage of the savings available in these short methods with no loss in sampling accuracy in those cases where a small number of additional cases can be secured to offset the slightly greater precision of the longer computational methods.

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NOTICE

Dr. Wayne Dennis will become editor of the *Psychological Bulletin* as of January 1, 1953. All new manuscripts should *henceforth* be sent to him at the following address:

PSYCHOLOGICAL BULLETIN

Dr. Wayne Dennis, Editor
Department of Psychology
Brooklyn College
Brooklyn, New York

The Book Review Editor will be Dr. Edward Girden. All books and other publications intended for listing or review should be addressed to him as follows:

PSYCHOLOGICAL BULLETIN

Dr. Edward Girden, Book Review Editor
Department of Psychology
Brooklyn College
Brooklyn, New York

BOOK REVIEWS

HELSON, H. (Ed.) *Theoretical foundations of psychology*. New York: Van Nostrand, 1951. Pp. xix+787. \$7.25.

For many, this book will have an exciting title. *Theoretical Foundations of Psychology* could be a book which examines the methodological presuppositions of psychological theory in the sense that inquiries into the "foundations" of mathematics perform a similar function for that discipline. Such a book is certainly needed. Or, the title might imply an assessment of emerging common properties within the untidy matrix of current theoretical efforts. All would welcome an attempt of this sort. The title could suggest a general comparative analysis of current theories. A craftsmanlike contribution of this order would be useful. Finally, the title might mark the intention to formulate, develop, or evaluate the "fundamentals" of some *particular* theory. Such an effort might at the least prove interesting. Actually, this lengthy symposium is all of these things and none of them; it is, in addition, occasionally a primer in general psychology, at times a supplementary survey of "fields of psychology," often a *Psychological Bulletin* review article, and, in one or two instances, a *salon* in which a contributor hangs a one-man show of his *Lebenswerke*.

Professor Helson does not quite succeed in preparing the reader for all of this in his *Preface*. We are informed that this "book is designed to treat the fundamental ideas, concepts, theories and problems which are at the center of the chief divisions of the subject." Further, the "volume should help advanced undergraduate and graduate students . . . to achieve some measure of integration of particular fields as well as psychology as a whole." Even the "mature psychologist, familiar with the best thinking in his own field will welcome . . . basic treatments of other fields." These aims were apparently realized by "the splendid cooperation of the contributors who . . . have succeeded in producing a volume fairly homogeneous as to level and type of material." To this reviewer, the only "homogeneity" evident in the volume was precisely in the single sense in which *heterogeneity* was promised. Despite Professor Guilford's comment in his brief *Foreword* (as general editor of the Van Nostrand series) that "we have passed beyond the school-forming era and that lines of affiliation are no longer clearly drawn," the one uniformity common to most of the seventeen contributors seems to be a predilection for some kind of theoretical framework of the sort variously called "field," or "neo-gestalt," or "dynamic," or "holistic."

It may prove instructive to approach this large, uneven volume as a kind of clinical exhibit of certain general trends in present "theoretical" psychology, and in psychological writing generally. As in any clinical

exhibit, such trends may be expected to emerge in exaggerated and prototypical form.

One trend which the book defines with disturbing impact is the superficial level at which vital methodological issues and topics from the philosophy of science are discussed. Those who have been a little concerned about the superficiality of much of the recent methodological literature can only find their anxieties confirmed within this volume. The tone is set in the first chapter, *Methodological Considerations*, by M. G. Preston. This chapter consists of an arbitrary stringing together of assertions about methodological and theoretical issues, half of them erroneous or distorted, and half deriving from the cliché-box of contemporary methodology. The tenor of the discussion may be inferred from the following sample of how major methodological concepts are dispatched: "When an assertion is made on the basis of faith it is called a postulate" (p. 7). Among the high lights of the presentation are a treatment of operational definition in which the operational criterion is nowhere specified, an obscurely formulated distinction between "reductionists" and "non-reductionists" which apparently represents the author's principal schema for the classification of psychological theorists, and a curious discrimination between "theories which presuppose inductive processes and theories which presuppose deductive processes" (p. 19). A treatment of independent, intervening, and dependent variables fails to distinguish between the senses in which "independent" and "dependent variables" designate data language specifications of the variables which are manipulated and recorded in concrete experiments (*experimental sense*), and theory language identifications of the chief empirical variable-classes which the theory is designed to relate (*theoretical sense*). This leads to the development of the somewhat labored theses that "Some dependent variables are called intervening variables" (pp. 16-18), and "intervening variables may be used as independent variables" (p. 19).

The first chapter presages the level of methodological analysis maintained in most of the others. Each author, however, adds his individual stamp in the rendering and evaluation of central methodological concepts. Terms like "molar," "molecular," "field," "intervening variable," "operational definition," "theory," etc., become chameleons which assume new semantic and emotive shadings in each chapter. Take the case of "operational definition." At one end of the spectrum is S. H. Bartley who, at the end of a chapter (*Fatigue and Efficiency*) devoted largely to a demonstration of the evils of regarding fatigue "as a narrow phenomenon to be defined 'operationally'" (p. 336), finally finds the *real* fatigue in the "layman's" knowledge of his "own feelings." At the other end of the spectrum lies J. Loewinger's chapter on *Intelligence* which, after dismissing as blind alleys the "philosophical," "pragmatic," and "factorial" solutions to the problem of intelligence, arrives at an "operational solution." Or take "intervening variables": Krech in his

chapter, *Psychological Theory and Social Psychology*, apparently feels that a wondrous reorientation of social psychology ensues immediately upon the remarkable intuition that "beliefs and attitudes" are "intervening variables"; for Beebe-Center (*Feeling and Emotion*) the "intervening variable" functions, in his extended analysis of the "construct" of "palatability," as a laboriously developed shorthand device for summarizing interrelations within a restricted domain of experimental data.

Attitudes towards "theory," in this remarkably comprehensive book, range all the way from Krech's, and MacKinnon and Maslow's (*Personality*) espousal of the global, if "programmatic" framework, to Harlow's (*Thinking*) comfortable basking in empiricism. Between these extremes, we find Helson (*Perception*) advocating a type of theory, instanced by the theory of "adaptation-level," which, though specified in scope, envisages the large aims of "supplementing," "broadening," and quantifying previous theories; we find also Beebe-Center, who seems on the side of the narrow scope systematization of highly restricted experimental domains.

More distressing than the range of the authors' attitudes towards theory, or their explicit views on problems of theory construction, are the occasional embodiments of these attitudes and views in concrete theoretical suggestions. For instance, E. B. Newman (*Learning*), after rejecting a series of one-paragraph résumés of contemporary theories of learning, offers to "make up the deficit [he] may have created" (p. 436), in a final section called *Suggestions for a Theory*. He does this in terms of six "theoretical considerations," each of which (p. 437) "might be called a principle, but actually . . . is more like a cluster of principles." Two of these "considerations"—"sensory selectivity" and "behavioral parsimony"—seem to mean what they ordinarily mean when used by other writers in the "field" tradition. Three others—"carry-over," "coalescence" and "response patterning"—seem to be words which, for some reason, are preferred to equivalent terms employed by field theorists, such as "traces," "trace organization," and "insight." The unique contribution is "motivational spread" which "represents an attempt to give a specific interpretation of . . . reinforcement" (p. 437), but the meaning of which requires further interpretation. How these six "principles," or "cluster(s) of principles" or "headings" or words are to be interrelated within the theory the author has in mind, what their systematic status is or is expected to become, are questions outside the scope of Newman's discussion. A comparably inchoate conception of theory seems implicit in Bartley's specification of a programmatic "theoretical structure" for the investigation of fatigue, and Krech's adumbration of a "perceptual theory" for social psychology. If the reader of this volume is likely to receive less than enlightenment by the talk about theory, his situation will not be improved by the "theorizing" actually attempted.

Another aspect of the book which seems disturbingly symptomatic

of a general shortcoming, not only in psychological writing and pedagogy, but in thinking as well, is the inadequate treatment or even total neglect of problems having to do with the interrelationships between each field and the others. The "advanced undergraduate and graduate students" who are trying to get some kind of view of "psychology as a whole" will, instead of achieving the "measure of integration" promised them, find only that their initial obfuscation is confirmed and amplified. Failure to consider problems of interrelationships in a sustained and rigorous way has consequences which transcend the merely pedagogical; any approach towards adequate theory demands, at every stage, tough-minded attempts to block out the possible ways in which discrete research areas mesh (or do not mesh) with each other and with the objectives of the science.

A book called *Theoretical Foundations of Psychology* should place a particular emphasis on meeting this obligation. Instead, we find at least four contributors—J. Oppenheimer (*Some Problems of Nervous Function*), E. Girden (*Some Neurological Correlates of Behavior*), N. Bayley (*Development and Maturation*), and J. Loevinger (*Intelligence*)—who make virtually no attempt to deal either with interrelations between their fields and others, or with the place of their specialties in the science as a whole. Of the remaining contributors, some (e.g., D. B. Klein on *Abnormal Psychology*, MacKinnon and Maslow on *Personality*, Harlow on *Thinking*) have made only a token effort to deal with interrelationship problems. Others (e.g., Newman on *Learning*, Helson on *Perception*) have touched on such problems in essentially stereotypical terms. A small minority—Krech and Beebe-Center—have subjected such questions to tortured, but somewhat empty, scrutiny.

While most of the authors slight interrelationship issues, each in his own characteristic way, it may be useful to discuss some concrete instances. Representative of the authors who neglect these issues almost entirely is Bayley, who proceeds to her survey of information about *Development and Maturation* after merely noting in a single sentence (p. 145) that "an understanding of psychological functions is greatly enhanced by knowledge about the processes through which these functions develop . . . with . . . age." Loevinger's excellent critical appraisal of factorial and other avenues to the analysis of intelligence could have gained in stature if, at some point, "intelligence" had been related to the problem of individual difference variables within more general theories of behavior, rather than approached as the circumscribed *Ding an sich* whose elusive ontological properties seem forever under pursuit by the investigators in this area. As representative of the contributors who provide only token analyses of interrelationship problems, we find MacKinnon and Maslow repeating endless variations on the theme that "investigations of personality . . . now make mandatory a revision of general psychological theory" (p. 602). At no point, how-

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ever, do we find concrete hints as to what these revisions may be, nor indeed what concrete relations do or should obtain between general psychology and personality. Newman, the symposium's expert on learning, and Helson, in his chapter on perception, offer us little more with respect to interrelationship problems than an emphasis, congenial to most with gestalt-like sympathies, on the intimate relations between the phenomena and principles of perception and learning—perception, of course, having systematic and functional priority. Krech, one of the few contributors who makes the assessment of interrelations central, emerges from some forty pages of text with the conclusion that "General psychology is social psychology" (p. 664), a conclusion which seems to confer another dimension of ambiguity on the analysis of the verb "to be" as treated in formal logic. Although this reviewer would guess at Krech's chief difficulty only with timidity, it is difficult to avoid the feeling that his discovery that all theoretical variables of general psychology are saturated with "sociality" overlooks the perfectly legitimate distinction between general laws and the conditions under which they operate. Beebe-Center devotes the substance of his contribution on feeling and emotion to a serious attempt to create a place for his topic within psychology. His earnestly documented theme is that progress in the conceptualization of feeling and emotion "during the last quarter of a century appears to have been the substitution of construction for reification" (p. 295). Nevertheless, when Beebe-Center comes to the "earthy" modern behavioristic theories of his preference, he fails to tell us precisely which elements of the constructions are relevant to the phenomena denoted by the now-outmoded reifications.

A third feature of the book, not without symptomatic significance for psychology in general, has to do with a complex of factors epitomized by the nature and level of the writing, by organization, and by style. Those who occasionally face the unhappy task of helping graduate students approximate to clear, scientific prose have reason to be worried by the prevalence of models similar to the present volume. The net impression created by the majority of chapters is that of turgid, imprecise, often vacuous writing. The fact that the personal flavor of the prose varies from the pseudo-Jamesian "Harvard-psychologese" ("memories seem . . . linked . . . like kaleidoscopic sausages in a giant string" [p. 395]; "I have . . . stretched the immensely complex past on a Procrustean bed" [p. 405]) of the chapter on learning, through the shabby rhetoric of the chapter on social psychology, to the inflated verbosity of the chapter on abnormal psychology, is secondary. More serious is the frequent failure to meet minimal standards of scientific or scholarly writing. At its worst, this may involve the failure to communicate anything; at its best the failure to communicate anything worth while.

Perhaps the most common form of this failing is in the "level"

at which experiments are summarized in the many "extant evidence" surveys and "relevant example" references which the authors feel compelled to include. Such summaries are nearly always far too skeletal, allusive, or vague to have meaning for those who are not already familiar with the study; nor do they serve better than the mere mention of the investigator's name to reactivate the memory traces of those who are familiar with the material. Of the chapters which include substantial reference to experimental materials, only the chapters on *Thinking* and *Some Problems of Nervous Function* manage to avoid this expendable level of summarizing. Equally, if not more pernicious, is the almost universal tendency to summarize complex theories in terms of an inexact translation of a single principle, or, at best, an impressionistic two-paragraph distillation of "essentials."

That these considerations of "style" are not irrelevant to content, can, I think, be proved by something like a random selection of quotations. One should suffice. The following passage appears somewhere in the book under the heading, "The Essential Unity of Psychology":

Irrespective of the difference in their views, Lashley and Hull are in agreement on the fact that psychologists, like all scientists, are engaged in the work of studying the interrelations among concepts, in particular, their functional interrelationships. Secondly, they are in agreement that the work of studying such functional relationships suffers in the extent to which it fails to meet the requirements of the experimental method. All psychologists are united in the study of behavior by means of the investigation of the functional dependence of concepts, through the use of the experimental method.

The tone of the present review should lend sincerity to some final comments about the happier ingredients of the volume. The really exceptional chapter in this book is Guilford and Comrey's judicious, knowledgeable introduction to *Measurement*. Instead of plunging into a statement of the formal conditions of measurement as in most primer presentations, the approach is to exhibit the significance of such formal criteria by first showing the distinction between pure and applied mathematical systems, and then treating measurement as the problem of finding specific empirical interpretations of the system of rational numbers. A second outstanding chapter—*Some Problems of Nervous Function*—is contributed by the symposium's only nonpsychologist, J. Oppenheimer. This well-organized, literate introduction to some foundation concepts of neurophysiology, with its refreshing emphasis on up-to-date research and its freedom from the usual stereotypes of the introductory text, should prove a useful reference in connection with serious undergraduate courses.

A number of additional chapters, although not untainted by the general trends previously noted, make, in some way, useful contributions. Despite Loevinger's failure to consider the problem of intelligence within a more general theoretical context, the bulk of her chapter

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is devoted to a skillful teasing out of the hidden apriorism of factor analytic approaches to intelligence. Helson's discussion of perception has the merit of some incisive analysis of a number of historical and recent theoretical issues. The outline of his theory of adaptation-level is, however, too schematic to be intelligible to any but those who, by virtue of previous knowledge, would find such a *précis* supererogatory. However slim Harlow's chapter may be in theoretical content, he does provide us with a useful, if selective, survey of research on the formation and functioning of "organized response patterns." It is regrettable that he nowhere tells us what "organized response patterns" means. F. W. Irwin's chapter on *Motivation* compares favorably with the chapters on this topic in most introductory texts, but is of uneven excellence. It suffers especially from a casual rendering of motivational "theories" and haphazard organization, but contains several thoughtful sections (e.g., the discussion of primary drives).

The word "theory" in contemporary psychology is a prestige word. It should be. In its best sense, it denotes a value which any science should be proud to work towards or attain. But in psychology and the social sciences this word is in danger of becoming a "prestige symbol" in the unhappy sense of the device used by advertisers to facilitate acceptance of a product, or by propagandists to produce feelings of awe. Detailed appraisal of a book so conspicuously unsatisfactory as the one under review may seem unnecessary or trivial. But at a time in psychology when nothing is more important than disciplined assessment of the preconditions to sound *theory*, it is fitting to question any further vulgarization of the term. It *could* just happen that someone might interpret *Theoretical Foundations of Psychology* as being relevant to theory.

SIGMUND KOCH.

Duke University.

MARX, MELVIN H. (Ed.) *Psychological theory*. New York: Macmillan, 1951. Pp. xi+585. \$5.00.

This volume is of interest not only for what it contains, but for raising the question of how we can best introduce students to psychological theory. Professor Marx proposes accomplishing this by means of a course built around a collection of readings from works of contemporary theorists.

In brief, the book contains selections from journal articles and books, organized under plausible section headings and threaded together by means of brief editorial comments. Part I includes selections concerned with methods and principles of theory construction in psychology while Part II is made up of representative writings from contemporary theoretical works.

The first part is planned "to encourage a more intimate understand-

ing and a sounder utilization of the principles of theory construction." These aims will certainly be at least partly realized. A number of well-known and influential papers—concerned, for example, with operationism, types of constructs used in psychology, levels of explanation, interbehavioral psychology, and applications of topology, factor analysis, operational analysis, and hypothetical-deductive methods in psychology—are made readily available to the student. The editor's introduction to this series of papers emphasizes the empirical basis of theory. He corrects the unhappy tendency of a number of psychological writers to identify operationism as a formal rule for defining acceptable concepts. Theories based upon infra-human research are defended vigorously. It is refreshing to see someone frankly take the stand that it may be respectable to study animal behavior simply with a view to understanding the behavior. This presentation may be better suited to engage the interest of the potential scientists in our classes than the common textbook justification of animal research as part of a broad program calculated to pay off in effective study habits, irresistible advertising methods, and popularity for everybody.

The notion of utilizing "principles of theory construction" is somewhat ambiguous. "Principles of criticism" would be more descriptive. It is safe to expect, however, that Part I of this volume, if it does not teach the reader how to construct theories, will at least influence him in the direction of being better able to appreciate good theories and less apt to publish bad ones. These readings will be good to have in the hands of students, especially since the editor has included the original lists of references with the reprinted papers, a device which may lure many readers into a more serious study of the method and philosophy of science.

The stated purpose of Part II is "to give the advanced student a certain amount of first-hand experience with the work of the various men who have laid the theoretical foundations for contemporary psychology within the past two decades." The twenty-four selections are drawn from the areas of perception, learning, psychodynamics (psychoanalysis, psychosomatics, psychological study of conflict and frustration), personality, and social interaction. The second part does not seem to be on a par either quantitatively or qualitatively with the first. The twenty-three selections in Part I average approximately sixteen pages in length; they are nearly all relatively self-contained papers that could readily form the bases for group discussion in classes or seminars. The twenty-four selections in Part II average only seven and one-half pages in length and are for the most part excerpts from larger works. These readings are said to be representative of the theorists in question. To illustrate the scale of sampling involved we note that the work of Koffka is represented by a nine-page excerpt from *Principles of Gestalt Psychology*, that of Freud by five pages from *The Problem of Anxiety*,

that of Skinner by approximately one-third of his early paper on the concept of the reflex. The array of theorists chosen for inclusion appears to reflect conventional standards quite accurately, although there are a few striking omissions. Some psychologists may not be happy with the impression that all theory of perception is gestalt or that there are no influential theories in the entire area of physiological psychology. On the other hand, it is not clear what could have been done to fill the gaps. The volume is large as it is, and the readings included could hardly have been cut much further without losing both syntax and sense.

The editor has deftly cut the ground from under potential critics of his selections by indicating in a Preface that they reflect his judgment neither with respect to "ultimate scientific significance" of the theorists nor to their value in exemplifying the view of theory construction presented in the papers of Part I. The product of this rather aloof editorial policy is an anthology that may be better fare for browsers among mature psychologists than for students receiving their first introduction to theorists and theories. It is doubtful that either theorists or theories can be usefully presented for instructional purposes by means of a strictly cross-sectional anthology of this sort. A scientific theory is an on-going activity, and its essential features are revealed only as it is seen in the process of development, modification, and interplay with experiment.

The possibility of developing the student's critical behavior is not strengthened by the plan of Part II. For on what grounds is the student to accept or reject the principles and points of view concerning theory construction presented in the first part? Presumably his judgment should be based upon observation of the consequences in actual practice when these principles are used or misused. But for this, we had as well skip Part II and send him to the library.

In general outline, this book reflects to some extent an accelerating shift in interest among psychologists from system building on a grand scale toward the development of more technical, experimentally oriented theories. The instructor who wishes to keep pace with this trend by developing new techniques for teaching psychological theory will find that Professor Marx has contributed a welcome experimental approach to the problem.

WILLIAM K. ESTES.

Indiana University.

MUNN, NORMAN L. *Psychology: The fundamentals of human adjustment*. 2nd edition. Boston: Houghton Mifflin, 1951. Pp. vxi+624. \$4.50.

The second edition of this now famous text is so nearly the same as the first (see Helson's review, this JOURNAL, 1947) that one wonders why Professor Munn felt it necessary to revise the book so soon. As an eclectic but scientifically sound work it has few competitors for use in one-semester courses of the general survey type.

To instructors attempting to introduce students to theory, however, the book's lack of theoretical discrimination is bound to be unsatisfying if not down-right irritating. Eclecticism in textbooks is essentially messy: neither the student nor the instructor ever quite gets closure on anything. Helson made a similar criticism on the first edition when he said it lacked an underlying point of view. This defect has not been corrected. Instead, Munn has added here and there some journalistic reporting of certain current theoretical issues. This is a good thing to try to do, if done well. Unfortunately, Munn doesn't do it well.

His big attempt is in the field of learning where he devotes eight pages to comparing the theories of Tolman and Hull. Reading these, the student will become confused rather than excited and the instructor will suspect that Munn doesn't understand the purpose of theory. He starts out with what seems to be a clear differentiation between the constructs of Hull and Tolman, but the distinction is lost as it becomes clear (to Munn) that Hull, after all, has only responses—and old ones at that—whereas Tolman has meanings and knowledge and cognitive maps. Gone are Hull's intervening variables in a wave of sentences like "One additional difficulty with the idea that *all learning is reducible to conditioned responses* is that, in order that a response may be conditioned, it must already be present" (p. 189, italics mine). A pseudo-resolution of the theoretical conflict is offered: both responses and knowledge are acquired in learning! One wishes Professor Munn had, after all, stuck to facts and never brought the matter up.

Incidentally, Munn has a new definition of psychology: it is the science of behavior, period. In the first book we studied "behavior and experience." We still do, really. That is, Munn is still quite as eclectic about subject matter as ever and by no means can the book be called behavioristic. Neither can it be called mentalistic, structuralist, gestalt, phenomenological or anything else, except possibly physiological.

So much for theory. The organization of the book is unchanged except for numerous new section headings, expanded introductions to the Parts and some condensation. The book now numbers 624 pages instead of 497 but this is an illusory increase in size; actually the type is bigger (and clearer) and most of the earlier fine-print sections have been incorporated into the regular-print running text—an admirable improvement. There is an absolute increase of 76 in the number of illustrations, 302 as against 226 in the first edition. Most of these are wonderful and many are new. In fact, the illustrations are the very best features of the book and help enormously to bring psychology alive.

A sample of other changes is the following: the facial vision studies now take precedence over the pipe-smoking experiment as the prototype of experimental method. A timely section on human engineering has been added. Unfortunately, the sections on neural summation and inhibition have been omitted and the section on retinal interaction

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abridged to meaninglessness, leaving, actually, no reference when page 435 (retinal interaction) says to refer to page 47 (neural summation) because page 47 no longer has anything to say on the matter. There is a new treatment of film and aperture modes of color but perhaps not enough to satisfy Helson. World War II data now replace old Army Alpha where necessary. The Wechsler-Bellevue is described and the measurement of infant IQ is properly added. Emotion as a conscious experience is deleted, in line with the "new" definition of psychology, I suppose. The new combination chapter on *Acquired Motives* is a competent rewrite job, but says little about theory of acquisition and neglects entirely the work of Miller and others, especially on fear and anxiety. But then Lewin and Maier are reduced almost to footnote status. Without counting words, I estimate the changes in the motivation part to be less than five per cent and this change probably is typical of the book as a whole.

One proper conclusion: if your bookstore is overstocked in the first edition, you can still feel safe in using it. Nevertheless, the new edition is a slight, though probably statistically insignificant improvement over the first edition.

In the face of valid criticism, why should anyone want to use the book as an introductory text? Because it is a fair picture of modern psychology. It is just about as sound and as confused, as brilliant and as dull, as heterogeneous and disorganized as the science of psychology itself is, in this year of 1952. It doesn't *bring* anything to our science; it just photographs it. The photo is not tinted with the charm of a William James; it is not as systematically narrow as a Watson or a Titchener; and it isn't as empty as Woodworth. It is a beautifully prepared and attractive document. Students like and respect it and most of what they learn from it will become a sound foundation for advanced study.

STANLEY B. WILLIAMS.

College of William and Mary.

STONE, CALVIN P. (Ed.) *Comparative psychology*. (3rd Ed.) New York: Prentice-Hall, 1951. Pp. xvii+525. \$6.00.

If Frank Beach is right in holding that comparative psychology in this country has vanished from having concentrated exclusively on the white rat (Snark, species Boojum), then this book goes only a little way towards resurrection of the departed. It is the third edition of the well-known *Comparative Psychology* previously edited by F. A. Moss. Contributing authors to the new edition are Nicholas E. Collias, Calvin S. Hall, Harry F. Harlow, William T. Heron, Donald G. Marquis, Henry W. Nissen, Robert A. Patton, Karl U. Smith, Kenneth W. Spence, Calvin P. Stone, Rolland H. Waters, Paul T. Young, and William C. Young—an imposing array of authors!

Some fine new sections on social and abnormal animal behavior have been added and the other chapters brought up to date, but the general format of the book remains unchanged. Individually written sections deal with such classical areas of psychology as maturation, motivation, learning theory, and discrimination. This should cause the true comparative psychologist immediately to suspect the snark: that despite the qualifying term "animal" before each section heading, here hides a general psychology text.

What is the difference? Well, unless we are all to be regarded as comparative psychologists (everyone works with some kind of animal), the term "comparative"—apparently a somewhat honorific term—should be reserved for those who *compare* the behavior of diverse animals, that is, do studies which in the words of Editor Stone "chiefly . . . are intended to bring out differences and similarities." The diversity of animals given coverage in this book is limited almost completely to mammals. And of these, the inevitable white rat and a few species of primates crowd all others off the stage. How much comparison is done within this small range of animals? Relatively little. For evidence, count the number of figures in the book which graph for us behavioral data of any kind. There are thirty-five. Now out of these, count the number which present simultaneously on the same set of axes data to be compared for more than one kind of animal. There is just one. Maybe this is too rigorous a test, yet the climate of the book is such that at least one section could be included (Spence's) that gives not the slightest suggestion that the subject matter ("Theoretical Interpretations of Learning") would possibly have phyletic parameters or provisos. Most of the sections could be transplanted unaltered into an advanced general psychology text without causing eyebrows to be lifted. This stems from the fact that the authors are generally topic oriented rather than animal oriented.

Aside from any possible mistitling of the book (and this after all may be unimportant), a striking feature of the work is its heterogeneity in style and level of difficulty. Flesch scores must range from the very low for sections by Hall and P. T. Young to very high for the sections by Marquis and Smith. The various authors were obviously aiming at different-level students. For example, Hall assumes that his readers need to be introduced to the general notions of reliability and validity of tests, whereas Smith assumes a reader knowledge of the distinctions between the psychophysical method of constant stimuli and method of limits. Heron explains that Pavlov worked with dogs and spells out the meaning of conditioned and unconditioned stimuli, while Harlow and Spence take it for granted that the reader knows what a Skinner box is. Nowhere in the whole book is the Skinner box (a natural habitat for *Mus norvegicus albinus*) described.

There are still things to be done with a heterogeneous book. The

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reviewer has found occasion to assign portions of it to his undergraduate students in experimental psychology, and other portions to graduate students in a learning seminar. This may reflect the secret of the book's continued success. Although it is not likely to be used as a text in a genuine course in comparative psychology, it is useful as a reference text in a lot of courses. The authors are all eminent writers who have something to say, and we want our students to read them even if the book that contains them is not "of a piece."

DAVID ZEAMAN.

University of Connecticut.

MILLER, GEORGE A. *Language and communication*. New York: McGraw-Hill, 1951. Pp. xiii+298. \$5.00.

The past decade has witnessed a rapid development of interest in communication problems, ranging from mathematical and engineering studies to large-scale studies on the effects of mass communications. For their own part, psychologists have become increasingly concerned with various aspects of language behavior—the nature of symbolic processes, intelligibility of messages, structure of verbal habits, and communication channels in groups of various kinds, to give only a partial listing. Courses on language behavior are beginning to appear on university curricula at both graduate and undergraduate levels. Yet we are severely handicapped by the lack of adequate texts and reference books in this area. The available books on language behavior are generally of ancient vintage and written from a frame of reference quite foreign to most contemporary students.

Miller's *Language and Communication* is a significant contribution as the first modern text in this awakening field. It is written by one of the most competent researchers on communication problems—Miller can speak with the authority of first-hand observation on many topics in this area. He makes comprehensible to the average student many implications of the mathematical communication theory developed by Shannon, Wiener, and others. As a source of information on what has been done and is being done by psychologists in studying language behavior, this will be an extremely useful book; there is no other compilation available with its breadth of coverage. This reviewer was also happy to find, early in the book, a brief but competent survey of linguistics; most psychologists have been oblivious to the contributions of linguistic analysis.

Miller lays down the general plan for his book in the introductory chapter: it is to be a *scientific* book on language, emphasizing quantitative data, and it is to be based on the mathematical model provided by Shannon's *information theory*. This blueprint is hewn to pretty closely throughout, but the emphases which develop at several points suggest that this model may not be entirely satisfactory. It is a very stimulating

approach, however, and one which encourages rigorous thinking. The first part of the book (Chs. 1-5) provides the student with basic tools for the study of language behavior, including excellent treatments of information theory, physiological and acoustical phonetics, speech perception, and statistical techniques of language analysis. The second part (Chs. 6-12) covers more "psychological" aspects of communication, including individual differences, development in the child, the structure of verbal habits (associations), the nature of symbolic processes, and social communication.

On the negative side of the ledger, this reviewer feels that *Language and Communication* is not a well-balanced presentation. Its quality is excellent in those areas where Miller himself is expert but sometimes rather thin in other areas; similarly, in quantity it is extremely detailed on topics where Miller has specialized but often skimpy elsewhere. For example, many pages are devoted to masking and other acoustical distortions despite Miller's own conclusion (p. 69) that "not until nearly all of the clues are eliminated does the listener falter"; yet the broadly significant topic of abstraction and concept formation receives only a scant two pages. The psycholinguistic problem of *decoding* (reception and interpretation of messages) is treated excellently and in detail, but the complementary psycholinguistic problem of *encoding* (selection and production of correctly coded messages) is hardly touched upon. This imbalance results in a highly variable level of difficulty for the reader. I imagine even most advanced graduate students will find the condensed materials on hearing very heavy going—there is insufficient redundancy!—whereas most of the materials on verbal habits and symbolic processes could be readily handled by the average undergraduate.

A serious weakness in this book, from the viewpoint of this reviewer, lies in Miller's handling of the role of learning (particularly in Ch. 8). He depends entirely upon the simple, one-stage S-R model developed by Skinner. Since this model takes no account of intervening, mediating variables, it is patently incapable of handling most human language phenomena. The picture one gets of the human communicator is one of a cluster of sheer labeling associations, external stimuli calling forth vocal noises with nothing in between. This reflects Miller's understandable attempt to be rigorously scientific and to deal only with direct observables, but I feel that it is possible to be equally rigorous with a more elaborate and flexible learning theory model that encompasses mediation processes. As a matter of fact, the limitations in the Skinnerian model become apparent from Miller's failure to use it systematically in later chapters on verbal learning and symbolic processes. Another outgrowth of the limited learning model is Miller's treatment of *meaning*—or better, lack of treatment. The entire problem of linguistic forms as *signs* having representational properties, of the development and measurement of significance, is missing. This also presumably reflects Miller's belief that we cannot be scientific and objective about meaning.

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In spite of these shortcomings, Miller offers a highly competent and cleanly written book on language behavior, whose objectivity is refreshing after plowing through the hazy murk of armchair theorizing that has characterized much of the writing in this field. As a textbook, it is factually rather than theoretically oriented and brings the reader into contact with a wealth of new and important material; as an exposition of a point of view, it illustrates how powerful an analytic tool the model developed by Shannon can be. I have included this text in a graduate course on the psychology of the communication process and it has proven to be a very valuable and stimulating addition.

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University of Illinois.

LUNEBURG, RUDOLF K. *Mathematical analysis of binocular vision*. Princeton: Published for the Dartmouth Eye Institute by Princeton Univer. Press, 1947. Pp. vi+104. \$2.50.

There are some advantages in reviewing a book five years after its appearance. By the end of that interval the reviewer can assess the initial effects of the work and take advantage of later expositions^{1,2} to provide a widened context for his evaluation.

Luneburg's book promises to have a great effect on theories of space perception. It has evoked considerable interest and promises to contribute significant findings through research that it has stimulated. Luneburg's premature death is a great loss to programs generated by his writings and to science in general.

The present review attempts to do two things: it first presents a summary of Luneburg's theory (together with an account of some experimental methods useful for evaluating parameters) and second, it analyzes some implications of the theory for a systematic study of behavior. Because we feel that it is desirable to present a rather full account, our summary is given in more than the usual detail. We use the slightly modified symbolism of the conclusion of Luneburg's book and of his later papers.^{1,2}

Luneburg's task in *Mathematical Analysis of Binocular Vision* is to determine the mapping of physical space into visual space and then to characterize the visual space. Both these tasks can be fulfilled if the metric of visual space can be expressed in physical coordinates.

The concept *visual space* requires elucidation. The visual space is a hypothetical space; Luneburg calls it the space of visual sensations. The visual space is not a physical space; it is not the space in which an observer manipulates experimental objects. When an observer says

¹ LUNEBURG, R. K. Metric methods in binocular visual perception. *Studies and Essays*. Courant Anniversary Volume. New York: Interscience Publishers, Inc., 1948.

² LUNEBURG, R. K. The metric of binocular visual space. *J. opt. Soc. Amer.*, 1950, 40, 627-642.

that one pair of points *looks* closer together than another, then in the visual space the first pair *is* closer together. The visual space differs from the continua usually obtained in psychological scaling procedures only in that it is three-dimensional.

If the mapping is determined, then an experimenter will be enabled to predict, from the physical coordinates of experimental objects, how an observer will say they appear with regard to form and localization, or, if the method of adjustment can be used, how the observer will set the experimental objects, i.e., their physical coordinates can be predicted.

A metric or distance function of a set of points is a non-negative number D satisfying three conditions. For any points, P_i , of the set: (1) $D(P_1, P_2) = D(P_2, P_1)$; (2) $D(P_1, P_2) = 0$ if and only if $P_1 = P_2$; and (3) $D(P_1, P_2) + D(P_2, P_3) \geq D(P_1, P_3)$.

The metric of any space is given in differential form by Riemann's generalized line element. For the special case of a three-dimensional space with constant curvature K , this is $ds = [(dx^2 + dy^2 + dz^2)^{1/2}] / [1 + \frac{1}{4}K(x^2 + y^2 + z^2)]$. Since the metric of a space determines all the properties of the space, the discovery of the metric of visual space will lead to its geometry.

Spaces represented by this differential, i.e., spaces with constant curvature, are the only spaces in which form and localization are uncorrelated, in which objects may be moved without suffering distortion in shape. Free movability, it may reasonably be assumed, is a characteristic of visual space. This conclusion restricts the nature of the visual space tremendously. If its curvature is constant, then Riemann's line element represents its geometry and there remain three possibilities: (1) $K=0$ (Euclidean geometry); (2) $K>0$ (elliptic geometry); or (3) $K<0$ (hyperbolic geometry). Which curvature adequately represents visual space can be answered only experimentally.

In addition to satisfying the three above conditions, the distance function of the visual space must have the two properties: (a) If P_1, P_2 and P_3, P_4 are two pairs of light points and an observer reports that the distance between P_1 and P_2 appears greater than the distance between P_3 and P_4 , then $D(P_1, P_2) > D(P_3, P_4)$; and (b) if an observer reports that the points P_1, P_2 and P_3 appear to lie on a straight line, then $D(P_1, P_2) + D(P_2, P_3) = D(P_1, P_3)$, and conversely.

The distance D satisfying both sets of conditions is the psychometric function Luneburg seeks. The problem is not only to determine this function but also, if possible, to express it in terms of the physical coordinates of the stimuli.

It may be shown that a distance function is determined only up to a linear transformation. That is to say, if $D(P_1, P_2)$ satisfies all the above conditions, then the function $\bar{D}(P_1, P_2)$ does also where, $\bar{D}(P_1, P_2) = C \cdot D(P_1, P_2)$, C being an arbitrary constant. This arbitrariness, the only one possible, means that there is no absolute sensation of size.

For convenience in representing the relationships between visual and physical space Luneburg introduces a bipolar coordinate system. In a cartesian coordinate system let the centers of rotation of the two eyes, R and L , be at the points $x=z=0$, $y=\pm 1$. If the eyes are fixated at a luminous point P^* , the optical axes of the eyes lie along RP^* and LP^* . Let the angle of elevation of the plane containing R , L , and P^* be represented by θ , angle LRP^* by α , and angle RLP^* by β . (See Fig. 1 of the reference of footnote 2.) The bipolar latitude $\phi = \frac{1}{2}(\beta - \alpha)$; and the bipolar parallax, $\gamma = \pi - \alpha - \beta$. (See Figs. 2 and 6 of the reference of footnote 2.)

The problem remains to find the relation between the physical space and the visual space, in which we may establish a cartesian coordinate system ξ , η , ζ , where the ξ , ζ -plane represents the subjective median plane, the ξ , η -plane the subjective horizontal plane, the η , ζ -plane the subjective frontal plane, and the point $\xi = \eta = \zeta = 0$, the apparent center of observation.

For infinitely distant points in the physical space, $\gamma = 0$. Since these points appear visually to form a dome of finite radius, they are represented in the ξ , η , ζ -space by a sphere concentric to the origin. Arbitrarily a sphere of radius 2 is chosen to represent physical infinity.

If a number of equidistant light points arranged on a Vieth-Müller horopter circle are observed, they will approximately appear to lie at equal distances on a circle concentric to the center of observation. A number of Vieth-Müller circles may now be constructed in the same way at various distances from the eyes. If this configuration is rotated about the y -axis, a Vieth-Müller torus is obtained, giving the impression of a sphere concentric to the center of observation. Expressions for points on this sphere are $\xi = f(\gamma) \cos \phi \cos \theta$, $\eta = f(\gamma) \sin \phi$, and $\zeta = f(\gamma) \cos \phi \sin \theta$; these functions relate visual and physical coordinates. The unknown function $f(\gamma)$ is a non-negative function of γ which increases monotonically if γ decreases from positive values to $\gamma = 0$. Because of the arbitrary choice, $f(0) = 2$.

Measures of $f(\gamma)$ obtained by means of Vieth-Müller circles² show that $\log f(\gamma)$ is a linear function of γ and therefore, $f(\gamma) = 2e^{-\sigma\gamma}$, where σ is a parameter having different values for different individuals. Expressed in bipolar coordinates, Riemann's line element can be written

$$ds = \frac{2}{e^{\sigma\gamma} + Ke^{-\sigma\gamma}} (\sigma^2 d\gamma^2 + d\phi^2 + \cos^2 \phi d\theta^2)^{1/2}.$$

"The constants σ and K are personal constants of the observer, . . . σ . . . determines the sensitivity of depth perception as compared with the perception of lateral size," while K determines the relation between visual estimation and physical size.¹

The Hillebrand-Blumenfeld alley problem provides a method for evaluating σ and K . The Hillebrand problem may be described as

follows: The subject arranges two rows of light points so that they seem to form parallel alleys symmetrical to the median plane. The Blumenfeld procedure requires the subject to arrange two rows of light points (in darkness) in such a way that all apparent distances between frontally coplanar points are equal. The settings made by the subject in these two situations form diverging curves that are not physically straight. This finding is not accounted for by a theory of corresponding points.

A method of determining σ and K is given by the fact that distance and parallel alleys become asymptotic at infinity. The equation describing the asymptote is $y = \tan \phi_0 (x + b)$; where $b_D = 2\sigma(1-K)/(1+K)$ (Blumenfeld); and $b_P = 2\sigma(1+K)/(1-K)$ (Hillebrand). Therefore, $4\sigma^2 = b_D \cdot b_P$ and $b_D/b_P = (1-K)^2/(1+K)^2$. This method of determining K is of course independent of any assumption regarding K 's being negative, i.e., K can be found in this manner without any assertion of a hyperbolic space.

The qualitative differences between the parallel and distance alleys demonstrate the hyperbolic nature of the visual space, but the method described is not convenient for measuring the parameters of the metric because the experimental technique is usually too elaborate.

Another estimation of σ may be made by observations of apparently right angles.¹ From the line element relation it follows that two line elements $d\gamma$, $d\phi$, $d\theta$ and $\delta\gamma$, $\delta\phi$, $\delta\theta$ attached to the same vertex will be discriminated as perpendicular if and only if $\sigma^2 d\gamma \delta\gamma + d\phi \delta\phi + \cos^2 \phi d\theta \delta\theta = 0$. If two such line elements are determined by experiment,

$$\sigma^2 = - \left(\frac{d\phi}{d\gamma} \frac{\delta\phi}{\delta\gamma} + \cos^2 \phi \frac{d\theta}{d\gamma} \frac{\delta\theta}{\delta\gamma} \right).$$

A better method for determining the parameters makes use of single observations of a pair of Vieth-Müller circles. The subject is instructed to equate the distance between the two comparison points on the near circle until it equals the distance between the standard points on the far circle. The method of evaluating the parameters is discussed in Luneburg's posthumous paper.²

Luneburg's theory provides a mathematical formulation which may be valuable in systematizing many types of observations.³ It is to be emphasized that the question of its value will depend upon the adequacy of its predictive power, a power that can be ascertained only when appropriately designed experiments have been carried out.

As it stands, the theory is essentially a formal account based on a mathematical model. It does not "explain." In fact, Luneburg disclaims any desire to account for the "mechanisms" of binocular spatial dis-

¹ Some calculations by Stein, based on an analysis that probably stems from Luneburg, may be important for the problem of visual "equivalence," e.g., in "distorted" rooms. Cf. A. Stein, A certain class of binocularly equivalent configurations. *J. opt. Soc. Amer.*, 1947, 37, 944-962.

criminations. The psychologist, therefore, can go on analyzing the determinants of space perception without necessarily relating his results to the Luneburg theory. Presumably, in the distant future a theory of space will exist that will take account of the (by then) demonstrated "mechanisms" of space perception. Such an account will provide us with a theory in terms of hypothetical constructs.⁴ Luneburg's theory is not of this sort; his visual space is a mathematical intervening variable.

The comment that Luneburg's theory is incomplete in failing to take account of distance cues other than stereoscopic factors is irrelevant, albeit true. Luneburg's theory is not a general theory of vision but a restricted one. To test the theory in an experimental situation the effect of other cues must be minimized—optimal viewing conditions cannot be provided.

Luneburg's concept of visual space holds considerable interest for systematic psychologists. There seems to be little doubt that Luneburg conceives visual space to be a phenomenological construct and the question arises: From the point of view of behavior study, what is the visual space to which Luneburg alludes? Very obviously what Luneburg's theory does is to relate some "setting" made by a subject (in the dimensions of Euclidean space) to a controlling magnitude of standard stimulus (also measured in Euclidean space). This type of experimental situation is describable by a formal relation of discrimination,⁵ $a_s = f(b, R_1, x_1, y_1, z_1)$ where a_s is the "equality" distance setting; b the standard distance; R_1 the reference response frequency; x_1 the constant effect of instructions; and y_1 and z_1 "conditions" of the organism that are essential ("mechanisms"?) to the discrimination. Thus, Luneburg's theory is a set of mathematical functions providing appropriate transformations to relate subject's "settings" to standard stimulus variables. It says nothing of "conditions" of the subject.

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P. RATOOSH.
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McCLELLAND, DAVID C. *Personality*. New York: William Sloane Associates, 1951. Pp. xvi+654. \$5.50.

It is puzzling to know what to say about this book. It has some woe-faults, and yet it makes some fine contributions. There are some flashes of keen thinking and some brilliant writing at a few points, as in the characterization of Freud in Chapter 1 and in some criticisms of Murray's concepts. The book gives a good critical discussion of Gordon Allport's ideas (though the criticisms sometimes side-step the

⁴ MACCORQUODALE, K., & MEEHL, P. E. On a distinction between hypothetical constructs and intervening variables. *Psychol. Rev.*, 1948, 55, 95-107.

⁵ GRAHAM, C. H. Behavior and the psychophysical methods: An analysis of some recent experiments. *Psychol. Rev.*, 1952, 59, 62-70.

questions raised by Allport); and it even more successfully builds on, and discusses critically, the over-all approach of Murray. A wealth of valuable research material is reviewed, both on animals and humans. A fair part of the latter is taken from studies by McClelland and his associates—studies which, to date, have not been adequately used elsewhere. Chapter 12, on experimental studies of motivation, gives the best critical discussion of motivation that I know of, even though I cannot agree with McClelland's final conclusion that only goal-expectations or anticipatory goal-reactions serve as motives.

McClelland has tried to give a theoretically-oriented rather than a practically-oriented discussion of personality, partly because (as he claims, p. xi) "theory must always precede application." To this end, he puts most emphasis on experimental studies (in a broad sense of the word), and a good deal of emphasis on TAT material (but not on most of the questionnaire-type personality tests because "... they are difficult to analyze in terms of any meaningful theoretical units," p. 186). Rather frequent quotations are made from a few clinical workers, particularly from Flügel as a spokesman for psychoanalysis—he has no aversion to using clinical theory as a source of hypotheses, at least.

The organization of the book is as follows: Chapter 1 outlines a thesis as to how mankind developed a faith in the possibility of a science of personality. It is an intriguing discussion, but "thin." Three next chapters deal with the methodology of research and theory-building. Three next chapters deal with "traits," which he describes at different points in rather different terms. Thus, sometimes traits are spoken of as "the characteristic way" a person has learned "to adapt to certain recurrent problems" (p. 156), and as "... consistencies in behavior in similar situations under similar motivation" (p. 230). Thus conceived, traits might include some rather fundamental characteristics, and McClelland might have discussed, as traits, the individual's ways of dealing with anxiety (per Freudian notions), and he ought to have described, say, Horney's hypotheses regarding the main patterns of social adjustment. But these Freudian concepts are described (at some length) only in a much later chapter, and Horney's type of hypothesis regarding traits does not appear in the book. Apparently there is a lot of truth in his statement that he "... decided for reasons of convenience to use the trait variable to describe the surface or stylistic manifestations of personality only" (p. 233; italics his).

Three next chapters discuss "schemata" (conceptions) and their development in the individual. Unfortunately, these chapters are in reverse order from their chronological reference to the individual's life. For class use, I would begin with Chapter 10 on learning problems of early childhood, then take Chapter 9 on roles and role models, and then Chapter 8 on the individual's learning of cultural ideologies.

The book ends with three chapters on motivation, one on the individual's conception of self, and a final chapter devoted mainly to a

further linking of the concepts of the book to an illustrative case used at length throughout the book.

As I said earlier, there are some very unfortunate features of the book. Let us start with a minor but troublesome one: There are no chapter summaries, no analytical table of contents, and (despite the title of Part V) no real summary chapter. What is more, the section headings within chapters are often poorly planned—supposedly coordinate points are not logically coordinate, etc. It is extremely difficult, in consequence, to get a clear idea of the main concepts of the book.

From the standpoint of use of this book as a textbook, one chief trouble is that the weakest and dullest part (Chapters 2-7 on methodology and traits) comes at the start of the book. Research is not actually done as he says, "obtaining the facts" first, "interpreting the facts" next, etc. What is more, research on personality has to deal with some grave problems which these chapters leave untouched. Later on, McClelland often stresses the idea that a person typically does not know many of his own characteristics and cannot report many of his personality processes. But there is nothing in the chapters on methodology to suggest that the important facts about personality do not just lie around on the surface of people's lives—that individuals often conceal, misrepresent, and have strong repressions which prevent them from revealing facts even when (in a sense) they want to cooperate with research workers. Indeed, it is significant that the rather full index of the book does not list at all the concepts of resistance, repression, or the Rogerian concept of self-defensiveness. It is true that the concept of repression is discussed under the term "withdrawal" in pp. 508-509, but the methodological sections have no benefit from such notions. The discussion of traits, on most scores, is very unsatisfactory. Any teacher using the book would be wise, I think, to skip these six chapters. I doubt whether a class could be resuscitated after struggling through this early part of the book.

Another fault is that the material is sometimes rather poorly digested, logically speaking. For instance, after trying to build a definition with some care, McClelland says: "... we have concluded with a definition of personality as the most adequate conceptualization of a person's behavior in all its detail that the scientist can give at a moment in time. ... a person's personality ... may change as he changes or as the scientist's insights improve" (pp. 69-70). Naturally, he doesn't stick to this definition. Such statements bespeak pseudo-sophistication—pretentiousness—not real thinking.

McClelland's concept about the place of motivation in personality is an unduly hampering one. Thus, he says: "... we need two concepts—one which will account for the consistencies and recurrences, and one which will account for the inconsistencies and sudden, irrational changes in behavior. For this purpose ... we may, following Murray, choose the terms *trait* and *motive*" (p. 215). Similarly, at the end of

the book, concluding his discussion of his long illustrative case, he says: "Perhaps 95 per cent or more of his responses . . . can be best accounted for in terms of his internalization of culture patterns (schemata) and his consistent modes of adjustment to recurrent problems (traits). Motives get to be decisive only in a small number of cases, which tend to get blown up beyond their real importance because they are unusual and dramatic" (p. 618). This view is regrettable. The chapters on motivation contain some of the best material in the book, but McClelland hardly has a framework in which he can use this material!

The chief foundation of personality concepts, McClelland says, should be our concepts of learning. But too often the learning concepts are half-understood ones (e.g., that Jost's laws signify that earlier learning leaves more enduring habits than later learning, pp. 341-342; and that "Extinction, as it has been observed in the laboratory . . . is so temporary that it could hardly be considered a serious reason for the disappearance of a trait like expansiveness" p. 219).

The long illustrative case of Karl, used in detail throughout the book, was not a case studied by McClelland, but was reported by one of his students on the basis of an autobiography, Strong Vocational Interest answers, Rorschach and TAT responses, perhaps one interview by a psychiatrist, answers to a sentence completion test, and a few other such things. The clinical workers are often berated by McClelland throughout the book. One could hardly blame them if they retaliated by criticizing this case as evidence of the shallowness of "scientific" studies of personality.

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University of Oregon.

EELLS, K., DAVIS, A., HAVIGHURST, R. J., HERRICK, V. E., & TYLER, R. *Intelligence and cultural differences*. Chicago: Univer. of Chicago Press, 1951. Pp. 388. \$5.00 (lithoprint).

First a word about the multiple authorship of this volume, 90 per cent of which was written by Eells. Herrick's name appears because of a six-page inadequate review of the literature. Both Davis and Tyler get in by rehashing previously published articles, and Havighurst's contribution is six pages. Thus future bibliographers are burdened unnecessarily with four extra names.

Eells reports herein his doctoral research on the relationship of cultural status to total scores and to performances on individual test items. Over 2200 nine- and ten-year-old pupils were tested on the Henmon-Nelson, Kuhlmann-Anderson, and Otis Alpha (verbal and nonverbal); and over 2400 thirteen- and fourteen-year-old pupils were tested on the Terman-McNemar, Otis Beta, Thurstone Spatial and Reasoning (from Primary Mental Abilities battery), and California Mental Maturity. These samples include practically all pupils of the

given ages in a city of about 100,000 population. Social status was determined on the basis of an index giving equal weight to parental occupation, parental education, house type, and dwelling area.

As expected, the relationships of total scores to the social status index were positive (correlations of .20 to .43), with the verbal tests showing the higher relationships.

The unique part of this research is the extensive item analysis, which is concerned mainly with the performance of a high-status group (top 10 per cent of the status index distribution) compared with that of a low-status group (bottom 15 per cent of distribution). The *N*'s for these comparisons are about 225 and 325 for the lower age group and about 235 and 360 for the higher age group. Of the 658 items involved in this analysis, 460 showed differences in percentages (passing) significant beyond the $p = .01$ level while 153 did not reach the .05 level of significance.

The chief finding is that the verbal type items, in general, yield the larger differences. Status differences in passing items are not dependent either on the position of the correct response or on type of question (analogies, classification, etc.). Status differences are related to item difficulties for verbal items, but for nonverbal items no generalization seems possible.

Two long and tedious chapters are devoted to a comparison of high- and low-status groups as regards correct and incorrect responses to certain items. Here one finds after-the-fact rationalizations in terms of differences in cultural backgrounds, but no explanation is found for a "rather substantial number of items" showing large status differences. There seems to be some dismay that certain supposedly culturally biased items did not yield status differences.

The study, though rather well executed and reported, is not free of questionable points. Pertinent differences in test *SD*'s are not considered (pp. 146-147; 155). The correlational term is ignored in computing certain standard errors of differences (pp. 114-117; 136-139). Subgroups are compared statistically with total groups without necessary allowance for lack of independence (p. 191). Inflated *N*'s are encountered (pp. 201-204).

Eells, perhaps in tune with his mentors, concludes that "variations in opportunity for familiarity with specific cultural words, objects, or processes required for answering the test items seem . . . to be the most adequate general explanation for most of the findings." He favors the construction of a common-culture test as a means of securing scores not subject to cultural bias. There are those who will say that Eells has not yet performed the crucial experiment since possible (and likely) genetic factors were uncontrolled.

QUINN McNEMAR.

Stanford University.

GURVITZ, MILTON S. *The dynamics of psychological testing*. New York: Grune & Stratton, 1951. Pp. xii+368. \$6.75.

This is a disappointing book. Its purpose is excellent: to present and analyze seventeen unselected cases which represent the routine work during one month of a clinical psychologist in a mental hospital. For each case we are given the diagnostic problem presented, complete protocols of all tests (including reproductions of figure drawings), analysis of the protocols, the psychologist's report (written before the case history was seen), the psychiatric therapist's comment on the validity of the statements in the psychologist's report, case history, and a final summary critique. There is an opening statement on the clinical use of tests which this reviewer found admirable and stimulating. The author then presents the discussion of the tests used. His comments on the Wechsler-Bellevue are adequate, but one often wishes that the field offered him more experimental data to back up some of the statements made. The sections on the Rorschach are disappointing. They are written much too briefly, on a level which will not be intelligible to the beginner, and do not offer enough discussion and elaboration for the experienced worker. The chapter on diagnostic formulations is sketchy and inadequate.

The case records are complete and interesting and will make excellent teaching material. The comments of the psychiatrist on the psychologist's report are interesting, but again sketchy. The flat acceptance of a single psychiatrist's statement of agreement or disagreement as an adequate criterion for the validity of the psychologist's interpretations is of dubious objective value. When these agreements and disagreements are counted and the chi-square formula is then applied, the result is an impression of statistical pretentiousness completely unwarranted by the inadequacies of the data. Scattered throughout are many pithy statements and illuminating comments, but the book as a whole suffers from loose organization. The publishers might well have supplemented the excellent format they have given the book by a tighter editorial rein on the author.

WILLIAM A. HUNT.

Northwestern University.

JANIS, IRVING L. *Air war and psychological stress: Psychological studies of bombing and civilian defense*. New York: McGraw-Hill, 1951. Pp. x+280. \$5.00.

This scholarly book is an eery sign of the times. In studies originally undertaken for the RAND Corporation and published under its *imprimatur*, Janis winnows the meager and inconsistent testimony from Hiroshima and Nagasaki and the richer though often impressionistic literature on civilian response to heavy bombardment in England, Germany, and Japan for their yield of empirical generalizations about psycholog-

ical reactions to attack by the "conventional" and "unconventional" weapons of modern air war. In his competent execution of this essentially historical task, he helps to secure for psychology the extended knowledge of human potentiality for adjustment that is a grim by-product of total war. From his critical survey of the evidence he then draws tentative conclusions about the underlying psychodynamics that should be of considerable interest to students of personality. The final sections of the book are concerned with psychological problems of civilian defense against atomic attack, partly by way of tentative policy recommendations, partly in terms of suggestions for needed research. The book is cast in a qualified, academic tone that contrasts with the appallingly unacademic facts of the past and assumptions about the future with which it deals.

Examination of published eye-witness accounts, together with a re-analysis of the original interviews from Hiroshima and Nagasaki conducted by the Morale Division of the U. S. Strategic Bombing Survey, leads Janis to reject the allegation that the atomic bomb produced any widespread panic, disorganized activity, or antisocial behavior. With the difference that all atomic survivors had experienced a "near miss"—rather than the minority as in the case of ordinary bombing—both the immediate and the more enduring psychological effects of atomic bombing seem to have been surprisingly similar to those of heavy conventional bombardment. The more adequate evidence available for conventional bombing thus becomes pertinent for assessing the factors that may influence psychological response to atomic attack.

The facts in broad outline about civilian adjustment to air war are generally known and accepted. Janis summarizes them thus:

Mental breakdown, panic, and mass demoralization—the triple psychological threats that dominated so much of the thinking in official quarters—rarely materialized during World War II. . . . the dire predictions made by many self-styled "experts" on mass behavior failed to take account of the psychological stamina of the average citizen. Bombing had little effect on the incidence of mental disorder; outbreaks of mass panic were rare; behavioral morale was maintained at a relatively high level. Although emotional shock did occur on a sizeable scale, most cases recovered fairly rapidly.

The details are more controversial, and Janis weighs judiciously the available evidence on such issues as the relative effectiveness on morale of distributed vs. saturation bombing, the apparent diminishing returns of the heaviest bombardment, and the predominance of intra-group vs. extragroup aggressive reactions.

Janis finds considerable support for MacCurdy's hypothesis that it is the experience of being a near-miss rather than danger as such that produces emotional disturbance; without near-misses, fear reactions tend to diminish during a series of air attacks. Such experiences, he offers in explanation, undermine the illusion of personal invulnerability which

gives effectiveness to many of the defense mechanisms for coping with anxiety.

Unlike the sections of the book that deal with the psychological effects of bombardment in World War II, which are well organized and comprehensive in their treatment, the concluding chapters on problems of civil defense against atomic attack are in the very nature of the case more sketchy. Among the problems treated are disaster control, recruiting and training civil defense volunteers, mass education for individual survival, and coping with apprehensiveness in potential target populations. The prime value of these chapters, which remain perhaps too close to the research memorandum stage for fullest usefulness to the general reader, seems to the reviewer to lie in their alertness to the complexity of the psychological problems involved in decisions of practical policy. When does "inoculation" to the horrors of atomic disaster inoculate, and when does it merely demoralize? How can civil defense teams be effectively motivated to train for disaster control work in neighborhoods or cities other than their own (for the teams in the target neighborhood cannot be expected to survive)? The problems, practical and theoretical, are immense, and they will be grappled with more intelligently on the basis of such considerations as Janis introduces.

M. BREWSTER SMITH.

Vassar College.

ROGERS, CARL R. *Client-centered therapy; its current practice, implications, and theory*. Boston: Houghton Mifflin, 1951. Pp. xii+560. \$4.00.

This volume covers the ten-year period since Rogers' *Counseling and Psychotherapy* was published. Besides copious material on individual cases and on studies of them, it has chapters on play therapy, group therapy, leadership and administration, and teaching, by members of the school who have had special experience in those applications of the therapy. There is also a chapter on a theory of personality and behavior, by Dr. Rogers.

Very early in the book there is a presentation of two striking and radical changes in point of view from the earlier expositions. The effectiveness of client-centered therapy is now said to be, first of all, a matter of the therapist's character: he must have a system of attitudes that is described at some length. The system of attitudes amounts to a profound belief in the sufficient (or unlimited?) capacity of a client to understand and reorganize himself on his own responsibility, not the therapist's. The problem of therapy then becomes: what is the best way of "implementing" these attitudes? Second, the counselor's role is now to convey understanding of attitudes and

feelings. More variety in attempts to do this is anticipated, to the point where the current formulation of the best way may be discarded. The current formulation, as I understand it, is that the therapist concentrates his whole effort on achieving a deep understanding of the private world of the client, and uses only responses that are attempts to convey understanding, excluding all others as far as possible, and conveying nothing deeper of his understanding than what the client perceives at that point. Although the door is opened to others, this is the only "implementation" treated to any great extent in the body of the book. It is curious that in a school which has prided itself on its vigor in research, and been admired for that, these striking changes should not have come as a result of its research. Yet they have not; they are laid to clinical intuition and experience. It is suggested that the experience was that cases grew longer along with the changes, and this involved an increasing degree of personality reorganization.

As I read along in the examples, illustrations, and citations of research, I was left feeling that there was a serious lack. The source of this came to light on pages 188 and 189, where I read of "our inability to profit in a research way from our failures," and "We have not been able to learn, in any general or significant way, from the clients whom we fail to help." I asked, "Then what is happening here?" We are told that a method works. We are shown devices accepted as legitimate samples of the method because they work. We would assume, then, that without them you get failures, and that with them you don't get failures. But it appears that the study of failures does not show this. Then what does "work" mean?

For that there are two tests: (1) The client's responses during the sessions. For example, a response shows insight, self-acceptance, further confession, and the like. This test is used, and goodness or badness of counselor activity is evaluated accordingly. (2) The client's success or failure in being helped. This one is not used, it fails to discriminate. That is, counselor activity (or client response?) cannot be shown to be different in the two. It would follow that the client's responses during the sessions are related to counselor activity, but that the client's success or failure in being helped is not related to counselor activity, or is not related to the client's responses during the sessions. I am not sure whether it is one or the other, or both. Should not the two tests be expected to have something in common, if they are to have importance in the long run? Although successes are said to be due to the use of non-judgmental speech and expression and of conveyances of understanding, failures are not due to their absence, or to their opposite. Until we know under what conditions a method fails, do we really know anything about why it succeeds when it does succeed? How else do we ever discover the illusory elements in our "successful" methods of doing anything? After all these pages, we seem to come

out where we have been before. Interest in the great mass of material begins to fade.

It is easy to be annoyed when people demand proof from this school of a kind that they never demand from their own, but this school has in a way tried to meet that challenge in its research. There is much to admire and agree with in the discussions of general and vitally important problems in the book, but essentially the book is an argument for a very particular method. It is the evidence on this that is disappointing.

DWIGHT W. MILES.

Western Reserve University.

WELFORD, A. T. *Skill and age. An experimental approach.* London: Oxford Univer. Press, 1951. Pp. ix+161. 8s. 6d.

The significance of the book by Welford and his associates is more in the development of a point of view and in methodology than in the actual findings presented. Thus the observation that with increasing age, there is a gradual reduction in performance of tasks involving "time-stress" will come as no surprise to those familiar with the experimental literature on aging—or to those familiar with old people. However, the emphasis on determining the relationship of age to the manner in which achievement is attained is of great significance. Furthermore, the presentation differs from most studies on aging in that a conceptual theory is developed, predictions are made on the basis of the theory, and results of experiments designed to test the theory are reported.

Although the importance of physiological factors is recognized, major emphasis is placed on the psychological and environmental aspects of the problem of age changes in performance. Thus, the differences between young and old in their methods of dealing with situations and problems is analyzed in some detail. It is postulated that the central factors of organization both of sensory data and response mechanisms are of primary importance in the diminished performance of the elderly.

The problem of sampling in the study of age changes is well recognized by the author and is met in a forthright manner, that is, by testing subjects he could get. Most of the subjects under 30 were students or naval ratings taking part in Service experiments in Cambridge. Subjects from 30 to 80 years of age were: (a) friends and acquaintances, (b) friends, tradesmen, and acquaintances recruited by a relative of one of the members of the research staff, (c) army officers obtained through the Cambridgeshire and Isle of Ely Territorial Army Association, (d) Polish army personnel and agricultural workers obtained from two Polish Resettlement Corps camps and two County Agricultural Executive Committee's Polish Hostels, (e) adult education classes and (f) members of old people's clubs. Although it is stated that age groups were equated with respect to socioeconomic status, education, and occupation, the evidence is not presented.

The experiments consisted of tests involving simple motor skills (such as tossing rings at a target), more complicated apparatus tests (tracking tests), tests requiring reversal of practiced skills (mirror drawing), tests of logical thinking on inspection tasks, and the solution of electrical problems. Many of the tests used were most ingenious and all were designed so that many partial scores, indicating the methods used by the subject in reaching a solution or completing a performance, could be obtained. Unfortunately the apparatus used is not described in sufficient detail to permit duplication by others.

Investigators hoping to find detailed observations on the regression of various test scores on age will be disappointed, since most of the results are analyzed in terms of differences between three to five broad age categories. The number of subjects tested varied from experiment to experiment, ranging from 12 to 75 per age group. The age groups compared also varied from "above 30" and "under 30" to 10-year intervals.

Some of the known psychomotor characteristics of old people are confirmed, such as the slower, more deliberate and accurate performance of older people, their greater susceptibility to confusion, their greater variability in test scores. However, the improved methodology of this study makes possible the development of a new concept, viz., that the impairment in performance in older people is not due only to peripheral, sensory, and motor impairments, but also to alterations in central factors concerned with the perceptual organization of sensory data and perhaps the central organization of responses. This is a valuable concept which will require more detailed investigation.

In addition to the theoretical implications of this study, there are a number of findings of immediate practical value. For instance, there is an indication that in training older workers, written instructions may be much more efficient than demonstrations. It is also evident that many well-learned and practical skills may be performed by older persons with as great efficiency and accuracy as the young. It is also suggested that older workers may perform more efficiently in groups of workers of their own age than when included among groups of younger workers.

The approach and methodology developed by Welford and his associates deserves careful consideration of all students of aging.

NATHAN W. SHOCK.

Baltimore City Hospitals.

BERRIEN, F. K. *Comments and cases on human relations.* New York: Harper, 1951. Pp. xi+500. \$4.50.

Inspired by contact with Dean Wallace B. Donham and his well-known Harvard course, Berrien has prepared a worth-while book on problems in human relations. Its level and content have been determined partly by the author's experience with a collaborative human relations course at Colgate University. It is a textbook pitched at the

level of college students and adult education classes; it is not an abstract, theoretical contribution which the publishers have called a text in order to promote sales.

The book's thesis is that "friction and discord in human relations can be reduced provided (1) we develop a clear understanding of the context and core of our conflicts in realistic rather than abstract terms; (2) we carefully assay the alternative lines of action in particular instances; (3) we attempt to predict the consequences of each alternative." Development of an explicit theory of human relations, says Berrien, is not an essential condition to improving human relations. However, "there are certain discoverable generalizations which describe the conditions underlying cooperative, satisfying group behavior"; these can be discovered through psychological and social science investigation.

Part I, the first half of the book, consists of "Comments" centering about aspects or conditions of behavior which affect social relationships. Chapters on "words and things" and on observing and thinking stress the importance of subjective interpretations, of inferences and generalizations, as affecting interpersonal relations. A section on motivation emphasizes the self-actualization drive. Frustration of this or other patterned motives may produce either aggressive or apathetic reactions. The importance of social skills and group codes and the problems caused by conflict between individual and group values are noted.

Part II consists of 28 case studies, running from those which emphasize the individual to those involving, in greater degree, relations between groups. These are actual life stories from a variety of settings—home, school, club, college, job, marriage. They typically end on an unfinished note, which facilitates discussion and suggests the need for more information. An instructor's appendix is designed to aid the teacher in taking a clinical point of view toward human relations problems.

Human relations courses seem to be taught by an extremely varied group of instructors; their emphases might not coincide with those of the author, who is known as an applied psychologist. I for one would have preferred more emphasis on emotional factors and on social roles as well as codes. Or again, I would favor stressing personality trends in leaders fully as much as in followers. But the bringing out of such differing interpretations is part of the task of class discussion, and an important aspect of the teaching process.

Curiously enough, Berrien does not indicate very clearly his frame of reference for interpreting problems in human relations. As already indicated, his viewpoint is not narrowly psychological. At times, however, the reader is puzzled. For example, the author makes a distinction, on the basis of complexity, between the kind of cases handled by social agencies and clinics and the kind he presents in the book. "The disturbances of human relations in everyday life," he says, "are relatively simple and consequently are amenable to interpretation without

the specialized, detailed information on mental level, endocrine balance, etc., that is essential for a clear understanding of the more puzzling and baffling problems confronting the clinic and the hospital." The relation between the two types of case, Berrien continues, is analogous to the relation between a doctor and a first-aid-er. "The situations dealt with in this course are of the first-aid variety. If properly handled, they will not need professional care. If bungled through ineptitude, they may fester into more serious problems" (pp. 466-467).

To me, this implies that seriously disturbed human relations problems have an organic basis, or at least derive from personality disorders, whereas everyday garden-variety difficulties are due to cultural or social influences of some sort. Yet a great deal of evidence could be cited to show that much of our divorce, delinquency, labor strife, and war itself stems from technological and economic changes or from various outmoded culture patterns. Furthermore, it is a real question whether everyday disturbances in human relations can be dealt with adequately by students briefly trained in social first aid.

For upperclassmen with a good background in psychology and social science this book may seem too simple. Most beginning students, however, will be stimulated to further exploration in a fascinating but relatively uncharted area.

S. STANSFELD SARGENT.

Barnard College, Columbia University.

ELLIS, ROBERT S. *Educational psychology: A problem approach*. New York: Van Nostrand, 1951. Pp. xi+546. \$5.00.

This is a well-written, well-organized book with clearly defined objectives which will meet a definite need in many colleges. Ellis differs from the authors of most recent educational psychology texts in believing that the educational psychologist should be a technical expert but not a policy maker. The psychologist's sole function in education, he believes, is "to point out the methods to be used in order to obtain the desired result with the greatest degree of efficiency." The objectives themselves are set up on the basis of philosophical and social considerations and should not be the concern of the psychologist.

Although the reviewer considers this position completely untenable, the resulting concentration on how to do what the schools are already trying to do has helped the author to produce a book which will be very valuable to teachers and to teachers-in-training. The problems referred to in the subtitle are dealt with under the following topics: the admission and classification of pupils, the curriculum, extracurricular activities, methods of teaching, the development of attitudes, tests and examinations, study methods, mental hygiene, exceptional children, counseling and guidance, discipline, and the problems of the teaching profession.

Since four and five years of professional training are becoming com-

mon there is a growing feeling that the teacher should be a professional in the most complete sense, that he should have such a clear understanding of human nature and of educational objectives that he can vary his practice with the situation and deal with situations which have never arisen before. Quite often, in our efforts to reach this ideal, our students acquire a fine inventory of psychological principles which they cannot quite connect with their specific teaching practices. Because he starts with quite definite problems Ellis, however, is able to work down to very specific applications. The following sentence, taken at random from the chapter on developing concepts, is an example of the detailed level of practice at which he consistently arrives: "In the case of both sound recordings and moving pictures there is one very important caution: it will be generally advisable that they be repeated several times in order that the desired observations may be more nearly complete and accurate."

The problems are discussed with fairness, good humor, and a conspicuous lack of dogmatism. Traditional as well as progressive procedures are given their due. The author is not one to throw out the baby with the bath; on one or two occasions he seems willing to leave a little dirt in the tub just to be safe.

His own point of view is consistent and helps the student to relate the various problems to one another. It is holistic, with the stress on meaningful learning as the chief educational process.

The topics are well documented, the relation between psychological theory and teaching methods is well developed, and the figures are not distorted to make a point. Although it may seem too good to be true, there is only one graph in which the zero point on the vertical axis is not shown.¹

Although this book could be used by a class which had no previous training in psychology, the author has intended that it follow a course in general psychology and a course in child psychology. It should be most effective and very helpful when used in that sequence and just before or just after student teaching.

DONALD SNYGG.

State University of New York, Teachers College, Oswego.

ENGLISH, HORACE B. *Child psychology*. New York: Henry Holt, 1951. Pp. xiv+561. \$4.50.

In 1941 appeared a paper-bound volume, *Studying the Individual School Child*, by Horace B. English and Victor Raimy. This was a unique contribution to the field of texts in child psychology. It was designed as a sort of guide to the student actually studying the child—a real child in his real environment. English and Raimy gave solid suggestions as to what to look for, how to go about getting essential information and, finally, how to evaluate what you have when you are through.

To this reviewer it is apparent that English has extended this general philosophy, developing now a book in child psychology. In fact, much of the meat of the earlier volume with Raimy has been incorporated bodily. *Child Psychology*, in this case, is a text developed around the idea that the student needs to work with a child, or children, and needs guidance in doing so. As English puts it in his preface, "If the choice lies between having the students read more, even of first-rate accounts of child behavior, or having them observe children, the latter is to be preferred" (p. ix).

From these introductory remarks it should be obvious that this text is not a source book, a bibliographic reference, or an account of what has been done experimentally or otherwise in the field of child psychology. It is a clear, readable presentation of material for the student on how to study children and why it is important to do this carefully. But suggestions are included regarding angles (all too often neglected and of real concern to workers in child psychology today) on the need for selling parents on the idea that *their* child should be studied, and on the special problems of teachers, who may want to be cooperative, but who simply may not have the time (or the principal's support) to give freely.

If you are teaching child psychology, as English would teach it, to people who want to develop a feel for the *child*, this will be an excellent text. If your special need is for an authoritative compendium of research on children, you will find this book unsatisfactory. The author is not unsound (he reveals a real knowledge of the basic literature in the field) but he is dedicated to the position that the student should study the child in the most effective way. If your course in child psychology has this orientation, you will find this a valuable text.

T. W. RICHARDS.

Northwestern University.

HORROCKS, JOHN E. *The psychology of adolescence*. Boston: Houghton Mifflin, 1951. Pp. xxvi+614. \$4.50.

This new textbook on adolescent psychology clearly makes an outstanding contribution to the literature in this field. The book is unusually well documented and draws heavily upon research studies. The 1,500 references which are cited not only include some of the best of the older research, but in addition cover in a very thorough manner the newer research of the past ten years. The book, however, is not a mere compilation of research studies. Horrocks has written a very thoughtful book and has succeeded in developing generalizations and an integrated viewpoint which is all too uncommon in texts in this field.

A particularly noteworthy feature of the book is its emphasis upon the social and cultural factors which influence adolescent behavior. Horrocks first devotes six chapters to a treatment of the social psychol-

ogy of adolescence before giving attention to the more traditional topics of intellectual and physical growth. Such an approach not only points up the fact that adolescence is primarily a social phenomenon, but also tends to make the book more interesting and readable for students. The book has good balance and no significant areas of knowledge are omitted that have relevance to an understanding of adolescent behavior. Unusually complete analysis is made of adolescent activities and interests in later chapters, and the book ends with two very valuable summarizing chapters. The first of these contains an integrated theory of human adjustment and the second is an extended and penetrating case study of an adolescent.

In a book as comprehensive as this one, certain errors inevitably creep in. For example, it is stated that "Kretschmer also proposes a deviate type which he calls athletic" (p. 292). Kretschmer's special or deviating types were instead designated "dysplastic." The athletic type represented one of his three major categories of body build. On page 289, McCloy is quoted, but the quotation is not contained in the reference which is cited. In general, however, the book seems to have been carefully prepared and to be relatively free of such faults.

In the main, Horrock's book is highly objective and modern in its approach and should do much to put the study of adolescence on a scientific basis. It furthermore is molar and functional in its outlook. Psychologists, counselors, teachers, and others who work with adolescents will find that the book contains much material that can be easily translated into practice. Students who have read the book as a text since the date of its publication have uniformly expressed favorable comments regarding its usefulness, completeness, and readability.

This text, in the opinion of the reviewer, is on the whole a superior production which will be widely used, and which will exert a very wholesome influence on the field of adolescent psychology.

GLENN M. BLAIR.

University of Illinois.

SYMONDS, P. M. *The ego and the self*. New York: Appleton-Century-Crofts, 1951. Pp. ix+229. \$2.50.

This charming little book is a sequel to *The Dynamics of Human Adjustment*, amplifying the approach to the concepts of the ego and the self that constituted a chapter in the previous volume. In a brief introduction, Symonds reviews some of the problems that arise in dealing with the ego and the self, from Kant to Gardner Murphy, Snygg and Combs, Frenkel-Brunswick, Werner Wolff, and Carl Rogers. That the relationship of the ego and the self is complicated, no one will deny. Symonds proposes to dispose of the confusion of the past by regarding the ego as objective, and the self as subjective. However, as he proceeds, with the ego, the self, the superego, the self-ideal, and the "indi-

vidual himself," all in the rôle sometimes of agents and sometimes of objects, the confusion between what is subjective and what is objective might lead the critical student into semantical and syntactical problems that would suggest to him that Symonds' solution is not so simple as it appeared to be at first.

The accounts of the structure, functions, and development of the ego and the self; the influence of self-feeling and self-evaluation on behavior; ego strength and weakness; the rôle of affects; the relations between the ego, superego, and self-ideal; ego breakdown and depersonalization; the rôle of the ego in pathological states; the place of the ego and self in psychotherapy; and Symonds' own suggestions about proper education as a means of arriving at satisfactory ego and self development—which constitute the contents of the volume—make interesting reading. Frequent references to concrete examples of behavior, particularly in children, lend an air of hard-headed practicality to even the most involved of the theoretical discussions and should go a long way toward convincing the reader that an individual's concept of himself must be reckoned with in any molar account of his behavior.

The point of view may well be called neo-Freudian, in that Symonds takes over the Freudian ego and superego, but introduces the self as an essential systematic concept. The id is not mentioned, though there is frequent reference to "basic needs and drives." Although Symonds warns against the dangers of reification and states specifically that the ego and the self are *processes*, he personalizes these "processes" so convincingly in his writing that only a relatively critical and discriminating reader could avoid finishing the book under any impression other than that Symonds has made out an excellent case for the belief that human behavior is controlled by the interpersonal relationships of a community of manikins within "the individual himself."

While engaging in polemics, Symonds does so in a kindly spirit that could hardly hurt anyone's feelings. Obviously fond of his manikins and their hosts, he imparts to the reader a kind of good will and tolerance for his fellow man that should be an asset in psychological counseling and psychotherapy.

The annotated bibliography should prove useful to many readers. As to the contention that the book "presents systematically and in organized fashion the results of experimental findings and indications from clinical experience concerning the ego and the self," it appears that perhaps 36 of the 211 titles refer to studies that might reasonably be considered to be experimental in nature, of which 16 concern the level of aspiration and level of achievement. Approximately 20 of the experimental studies are referred to in the book proper. Nothing by Harry Stack Sullivan is included in the bibliography and his theory of the development of the self is not mentioned as such.

ARTHUR JENNESS.

Williams College.

CULBERTSON, JAMES T. *Consciousness and behavior*. Dubuque, Iowa: Wm. C. Brown, 1951. Pp. vii+210. \$4.50.

The purpose of this book is to present, from a strictly mechanistic point of view, a neural analysis of consciousness and behavior. To describe the relationship between input (stimuli) and output (responses) of human and animal machines, the author constructs systems of *throughput* in the form of hypothetical nerve nets, which represent patterns of neural interaction. Basic in accomplishing this are postulates of neural transmission and quantized time, standard in the field of mathematical biophysics. These postulates, though overly simplified, allow simple symbolic logic to be used in making statements describing nerve nets and can be easily replaced by the more accurate, but more complex, postulates of experimental physiology.

While his basic approach is quite similar to that of other workers in the field, such as Rashevsky, Pitts, McCulloch, and Landahl, the author diverges rather widely from them in terms of his ultimate objective. Instead of being concerned with establishing general principles of neural interaction, Culbertson becomes more interested in constructing nerve nets which account on this level for specific psychological problems. For instance, he presents nerve nets that could possibly account for the "Gestalt problem," the phi phenomenon, the ability to time events, and the capacity for memory. One area in which he shows even greater divergence from other mathematical biophysicists is that of the mind-body problem. Where such thinkers as Rashevsky refuse to admit a phenomenon of consciousness, Culbertson takes the view that consciousness cannot be ignored and seeks to account for it as a by-product of the interaction of neural impulses as they proceed from receptors to effectors. He thus offers neurological bases for a *psychospace* and for sensory intensity and quality. That these nerve nets do not constitute actual explanations is readily apparent. They are offered, rather, as possible neural connections underlying different types of psychological phenomena. It is then up to the psychologist to specify those characteristics of nerve nets which will be compatible with his empirical observations and thereby help select those that actually obtain in organisms. Once the proper nerve nets have been determined, behavioral characteristics can be further delimited and the psychologist is aided in predicting responses.

The psychologist who does not like to spend hours wading through complicated formulae will be glad to know that this book, unlike most in the field of mathematical biophysics, contains a minimum of logical constructions, and these are quite ably explained in the earlier sections. For such a complex area of study the text is clearly and simply written and should prove fruitful for use in classes in psychological theory or in more advanced classes in physiological psychology.

F. J. MCGUIGAN.

Human Resources Research Office, Department of the Army.

BOOK REVIEWS

Symposia of the Society for Experimental Biology. IV. Physiological mechanisms in animal behavior. New York: Academic Press, 1951. Pp. vi+482. \$6.00.

This volume contains 21 papers read at a Symposium of the Society for Experimental Biology, which was held at Cambridge University in July 1949. The program was subdivided as follows: "The Range and Capabilities of the Sense Organs," 4 papers; "Central and Peripheral Control of Behaviour Patterns," 5 papers; "Instincts, Taxes, etc.," 8 papers; and "Learning," 4 papers.

Of the 22 contributors, representing five different nationalities, two are Americans and only one is a psychologist. Some of the papers are of outstanding quality. Erich von Holst, of the Max-Planck-Institute in Wilhelmshaven reports a series of truly beautiful experiments designed to elucidate the sensory control of positional responses in fishes. B. B. Boycott and J. Z. Young describe an impressive experimental program dealing with neural mechanisms involved in cephalopod learning. K. S. Lashley's stimulating analysis of the current status of various neurological theories of learning serves chiefly, as he concludes, to show "what and where the memory trace is not." It is a considerably more convincing evaluation of current knowledge than is that of J. Konorski, who continues to think of conditioning as resulting from the formation of connections between different brain centers.

The papers vary a great deal in quality, but, taken as a whole, the volume should be extremely useful to American psychologists. Laboratory research workers may feel that some of the investigations carried out by biologists are less thoroughly controlled and less cleanly designed than might be wished. Certainly some of the zoologists display a lamentable lack of sophistication in the conceptualization of psychological problems, and there is an occasional tendency toward philosophical disputation.

Much more significant than any negative criticism, however, is the fact that the study of behavior is being pressed forward upon so broad a front in other countries. It is noteworthy, also, that this work is being done, not by psychologists but by biologically trained investigators. In many respects they are students of behavior in a truer sense than are professional psychologists. Certainly no one could read this collection of papers and escape the conviction that a genuinely comparative approach to psychological problems is fruitful and worth while.

FRANK A. BEACH.

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CULBERTSON, JAMES T. *Consciousness and behavior*. Dubuque, Iowa: Wm. C. Brown, 1951. Pp. vii+210. \$4.50.

The purpose of this book is to present, from a strictly mechanistic point of view, a neural analysis of consciousness and behavior. To describe the relationship between input (stimuli) and output (responses) of human and animal machines, the author constructs systems of *throughput* in the form of hypothetical nerve nets, which represent patterns of neural interaction. Basic in accomplishing this are postulates of neural transmission and quantized time, standard in the field of mathematical biophysics. These postulates, though overly simplified, allow simple symbolic logic to be used in making statements describing nerve nets and can be easily replaced by the more accurate, but more complex, postulates of experimental physiology.

While his basic approach is quite similar to that of other workers in the field, such as Rashevsky, Pitts, McCulloch, and Landahl, the author diverges rather widely from them in terms of his ultimate objective. Instead of being concerned with establishing general principles of neural interaction, Culbertson becomes more interested in constructing nerve nets which account on this level for specific psychological problems. For instance, he presents nerve nets that could possibly account for the "Gestalt problem," the phi phenomenon, the ability to time events, and the capacity for memory. One area in which he shows even greater divergence from other mathematical biophysicists is that of the mind-body problem. Where such thinkers as Rashevsky refuse to admit a phenomenon of consciousness, Culbertson takes the view that consciousness cannot be ignored and seeks to account for it as a by-product of the interaction of neural impulses as they proceed from receptors to effectors. He thus offers neurological bases for a *psychospace* and for sensory intensity and quality. That these nerve nets do not constitute actual explanations is readily apparent. They are offered, rather, as possible neural connections underlying different types of psychological phenomena. It is then up to the psychologist to specify those characteristics of nerve nets which will be compatible with his empirical observations and thereby help select those that actually obtain in organisms. Once the proper nerve nets have been determined, behavioral characteristics can be further delimited and the psychologist is aided in predicting responses.

The psychologist who does not like to spend hours wading through complicated formulae will be glad to know that this book, unlike most in the field of mathematical biophysics, contains a minimum of logical constructions, and these are quite ably explained in the earlier sections. For such a complex area of study the text is clearly and simply written and should prove fruitful for use in classes in psychological theory or in more advanced classes in physiological psychology.

F. J. McGUIGAN.

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